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ROBERT WHITE McFARLAND.*

W. W. PAYNE.

It is a pleasure and a privilege to give to the readers of POPULAR ASTRONOMY a brief account, in appreciation, of the active life and the great service to science of our personal friend, Dr. R. W. McFarland, former President of Miami University at Oxford, Ohio, who went to his final rest Sunday morning, October 23, 1910, at the advanced age of 85 years. His life had been singularly free from illness of any kind or physical infirmity until after he had passed his eightieth birthday when he was heard to say with a twinkle in his eye:—"I have a disease that has come to stay—old age."

Dr. Robert White McFarland of stalwart Scotch ancestry was born near Urbana, Ohio, June 16, 1825. He attended a district country school, and at the age of fourteen he received a certificate to teach. He began his life career of nearly fifty years, as teacher, at that youthful age in Miami County, Ohio. One who knew him intimately has recently said of him that he was "a born mathematician." "He was at thirteen familiar with surveying and well advanced in Algebra and Geometry. While teaching at Westerville, Ohio, in 1843, being anxious to see a college, he walked twenty-five miles to Granville; and when tramping back, made the resolution to go to college. In the spring of 1845 he went to Delaware, Ohio, and entered college. At an exhibition at the close of the term young McFarland delivered his first public address at the Ohio Wesleyan University where he graduated in 1847. After teaching

* It was an oversight, greatly to be regretted, that information of the death of Dr. McFarland did not reach the writer at Elgin, Ill., until the last days of February, 1911. This will account for the late appearance of this article.

The accompanying picture of Dr. McFarland is the only good one that could be furnished by a near relative of the family. Though not a late one it is a faithful likeness.

a short time at Delaware, he filled an important position in the Greenfield Seminary, Highland county, O., from 1848 to 1851."

"At Greenfield, March 19, 1851, he was married to Miss Mary, second daughter of the late Judge Hugh Smart at that place; a lady of rare beauty, refinement, and sweetness of character, whose sudden death, February 23, 1907, at Jacksonville, Florida (where with her husband and daughter, Miss Elizabeth, she had gone for the winter, leaving her daughter, Mrs. Fannie Bonham and her husband to care for the home) was a most painful shock to her devoted family, relatives and friends."

Professor McFarland came from Madison College, Guernsey County, O., where he was professor of mathematics, to the chair of Mathematics and Astronomy in Miami in July 1856, in which his work was very successful until the close of the University in 1873.

In no service was Professor McFarland more affectionately remembered by his old students than as their Captain and Colonel in the Civil War. For his patriotism and his unselfishness in sharing their exposure and hardships and for his kindly companionship and tender sympathetic care in all their trials. An instance of this was the capture, September 9, 1863, of Cumberland Gap, in East Tennessee by "Mac" Brigade against great odds—on the march out of the Gap, Col. McFarland, having charge of the 86th O. V. I. gave up his horse for the tired boys of the rank to ride, and himself walked seventy miles of the march. In after years, when visiting Miami, the first thought of the old boys was of their brave and considerate Colonel, and their first care was to call on him and talk of the times that tried men's souls. As a soldier of his country, as well as a professor in the University, he was faithful, efficient and true."

"In 1873 Professor McFarland was elected to the chair of Mathematics, Astronomy and Civil Engineering in the Ohio State University at Columbus, where he remained for twelve years, having most of the time an assistant in each department. On the reopening of Miami in 1885, Professor McFarland under urgent solicitation to accept the presidency, reluctantly gave up his fine position at Columbus, and returned to Oxford where he labored hard for three years, at a critical time and under great difficulties, at the head of the University."

"In 1888 he resigned the presidency of Miami after a service there for twenty years, and accepted a position as Mining

Engineer and manager of large real estate interests for a coal company in Hocking Valley, Ohio."

"It was especially gratifying to Dr. McFarland, that, on his 80th anniversary, June 16, 1905, a mathematical prize was founded in his honor, by one of his old students, which when awarded and publicly announced each year at the commencement of the University, and the continued mention of it in the annual catalogues of the institution will perpetuate his name and memory during the existence of Miami University."

We have given this brief outline of the life-work of Dr. McFarland from the ready pen of another, as the basis of a few more words of appreciation which we are delighted to add from known reminiscences and a personal acquaintance for more than thirty years.

Referring to his active life and eminent services as a college professor in Miami University, we can say, he was one of eight members of the Faculty of the institution that made a notable company of instructors who were able to leave the impress of their characters and their work in a rare and potent degree on the many students fortunate enough to study under them. Those men possessed general and comprehensive learning, and they had won for themselves strong individuality and a unique force of character, enthusiastically adapted to their teaching and their work.

Many of the readers of this magazine will remember these men, as their names are mentioned, who were associated with Dr. McFarland in Miami University, during the period before mentioned, because of the enviable reputation which they then and afterward acquired in their chosen fields of labor. One was the revered Dr. John Hall, President, who taught well in eight different branches associated with the evidences of Christianity. Another the Rev. Dr. Charles Elliott, teacher of Greek with eight other branches. Another, Professor Robert H. Bishop, said to have been class-room mind-reader, teacher of Latin and three other branches. Professor O. N. Stoddard, enthusiastic teacher of Natural Philosophy and six other branches, and Professor David Swing teacher of English Literature with two other branches. All these men received the honorary degree of Doctor of Laws from various institutions because of wide research, professional ability and national reputation on account of their advancement and success in some lines of very difficult research.

Dr. McFarland was not only broad and painstaking in

his scholarship, but he was also a writer of note-worthy ability. The accuracy of his knowledge of the Latin is known by the edition of Virgil which he brought out in 1846, including six books of the Aeneid and three Eclogues. His aid was also secured in revising Loomis' Algebra and Robinson's text-books in Mathematics. He was greatly interested in History and Archaeology and frequently wrote for the "Ohio State Archaeological and Historical Quarterly." A few of such themes that claimed his attention were: "Ancient Earthworks, Oxford;" "Forts Laramie and Pickawillany;" "Notes, Geographical;" "Historical Notes;" "The Chillicothes;" "Simon Kenton;" "Ludlow's Line."

His exact and thorough knowledge of the higher Mathematics was very useful in his study of the great problems in practical Astronomy. His most laborious work in this field was his computation of the eccentricity of the Earth's orbit and the longitude of its perihelion, two of the very important elements of the Earth's annual path around the Sun. The time he gave to this difficult work was four hours per day, six days in the week for four years, (1876-1880). A bit of history here will show the importance of this work and Professor McFarland's interest in it. James Croll (of H. M. Geological Survey, Scotland) in his "Climate and Cosmology," 1885, "had used the form of the Earth's orbit in his theory of the Ice Age. The late Dr. Orton asked Dr. McFarland whether Croll's astronomical work could be relied on; if so, the presence of boulders over Ohio and other states could be fully explained. Croll had computed the form of the orbit at intervals of 50,000 years, over a period of 3,000,000 years. Meanwhile the astronomer, Newcomb, (Washington, D. C.), had said that Croll's work could not be trusted but that Stockwell's (Cleveland, O.,) could be.

Dr. McFarland computed the form of the Earth's orbit by both Stockwell's and LeVerrier's methods for over 4,500,000 years at the short intervals of 10,000 years, and showed that the two were in substantial agreement for the entire time. When the two curves were platted, they were very much alike—no difference for 70,000 years."

The work of A. Blytt (Sweden), presented in the Smithsonian Report of 1889, On the Movements of the Earth's Crust, gives the following:—

"The curve of the eccentricity of the Earth's orbit has been calculated by J. Croll for a period of 4,000,000 years, 3,000,000 years backward and 1,000,000 years forward from the present

time."

"The curve is also calculated by McFarland (Am. Jour. Sci. 1880-3, XX. pp. 105-111). His calculation extends from 3,250,000 years backward to 1,250,000 years forward in time."

"He has calculated with shorter intervals than Croll (Croll 50,000, McFarland 10,000) which however, has had no influence in altering the curves. McFarland has in the same place calculated the curve for the same period of time from new formulae by Stockwell."

"The two curves taken in the gross show a uniform course throughout their length, but as regards the first half of Le-Verrrier's curve it is thrown somewhat backward. Stockwell's formulae are considered somewhat more accurate than Le-Verrier's."

"Both curves are given by McFarland. If we compare these two it appears:

(1) The two curves coincide with only a small essential difference from the present day until 1,000,000 years back.

(2) A very remarkable consequence comes from the calculations. *The curve repeats itself* after a lapse of 1,400,000 years when calculated by Stockwell's formulae. In the period of 4,500,000 for which McFarland has calculated it, the repetition occurs in this way with remarkable regularity a little more than three times."

While Professor Newcomb of Washington questioned both Croll's and LeVerrier's work, he acknowledged the accuracy of that of Dr. McFarland. He says of it, "I may here mention that Professor McFarland of the Ohio State University at Columbus, a few years ago, undertook the task of recomputing every one of the one hundred and fifty periods given in my tables, and he states that, except in one instance, he did not find an error to the amount of 0.001."

"In this laborious undertaking Dr. McFarland computed by means of both formulae the eccentricity of the Earth's orbit and the longitude of perihelion for no fewer than 485 separate epochs." See Am. Jour. Sci. Vol. XX, p. 105, 1880.

From what has been said of Dr. McFarland's success as a scholar, a teacher of Mathematics, Astronomy and kindred branches or as a foremost practical astronomer it cannot for one moment be rightly supposed that his scholarship was less in kind or quality than that of those distinguished men just mentioned; although it is so often said by some people that mathematicians and astronomers are not broad and general scholars, on ac-

count of the nature and influence of the studies they pursue. The idea that Mathematics and Astronomy, the oldest and the most exact of all sciences known to man should have even the tendency to narrowness of thinking, or lack of breadth in comprehension, or the want of grasp in thinking power for terse logic is simply preposterous. Dr. McFarland's noble life is a complete answer to any such erroneous view.

For more than fifty years Dr. McFarland wrote on astronomical subjects for popular and scientific magazines, such as the "American Journal of Science," "Open Court," "Sidereal Messenger," "Popular Astronomy," and various others, in which he was always an able and a very welcome contributor.

It was in this connection, while the writer was conducting the "Sidereal Messenger," in 1882 that an acquaintance with Dr. McFarland was made, through the articles which he prepared at that time, and later, for the magazine and for this one. In the twenty-five communications that he wrote for these publications, the writer's acquaintance ripened into a friendship for him that put him among the choicest in all the wide circle of his scientific friends. He was so highly esteemed and his scholarly judgment deemed so reliable that he was often consulted in regard to matters of doubtful authority by the writer, who was never sorry or uncertain in adopting the wise counsel of his amiable friend and fellow scientist.

THE TRUTH ABOUT THE GYROSCOPE.

HYLAND C. KIRK.

That a mystery is involved in the action of the gyroscope and also the common top, whose motions are similarly affected by gravity, has long been understood; and yet where such movements are plain to be seen, there is no good reason why they should not be perfectly clear and comprehensible. The distinguished French scientist and editor of *La Nature*, Gaston Tissandier, has said of the former, "The gyroscope, though now an instrument common and familiar to all students, is none the less the subject of a problem which is still to be solved"; and an eminent English engineer, John Perry, says

of the latter in his work on *Spinning Tops*, "All so well known as the rising tendency of a top has been ever since tops were first spun, I question its being known to more than a few persons anywhere. Any great mathematician will tell you that the explanation is surely to be found published in *Routh*, or that at all events he knows men at Cambridge who surely know it, and he thinks he himself must have known it, although he has now forgotten those elaborate mathematical demonstrations which he once exercised his mind upon. I believe all such statements are made in error, but I cannot be sure." (Spinning Tops, pp. 67 & 68.)

Though many distinguished scientists, as Phillips, Faraday, Secchi, Stewart, Tait, Mohr, Maxwell, Challis, Guthrie, DuBois, Reymond, Croll, Preston and Dolbear, have for one reason or another reached the conclusion that the phenomena of gravitation are not due to an attraction in matter but rather to a pressure from space, yet no experimental demonstration of gravitative action has hitherto been observed or pointed out so clearly as to confirm this view. Yet the action of the gyroscope and common top undoubtedly present such evidence which may be seen by any intelligent person who will take the pains to observe their movements and note their meaning.

If the disc of a gyroscope as shown in figure 1 is set in rapid

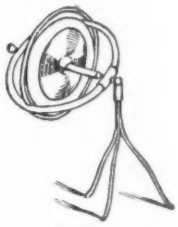

FIGURE 1.

rotation,* and the projecting arm placed upon a pedestal, the instrument left otherwise unsupported begins a circuit around the pedestal which is maintained until the force of its rotation is exhausted. A peculiarity of this movement is that, if the disc as observed from above rotates in one direction, it revolves around the pedestal in opposite direction. Two questions arise here: What causes the disc apparently to support itself, and why does it always revolve around the pedestal in

* *Rotation* is this paper refers to the disc's movement on its center *revolution* to its movement around the pedestal.

the direction of its rotation at its base?

In answering the first of these, the fact that, if the entire apparatus is placed on a weighing-scale while the disc is rotating, it weighs precisely the same as when at rest shows that the disc exerts no real uplifting power in its motion; while the fact, if the arm resting on the pedestal is slightly flexible, it will be bent downward while the instrument is revolving (Fig. 2)

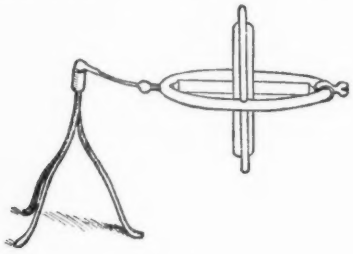

FIGURE 2.

shows that the whole weight of the disc, though apparently supporting itself in air, is really thrown on the pedestal. Accordingly a third question arises: In what way is the weight of the rotating disc and its carriage exerted upon the arm while revolving around the supporting center so as to be upheld by the latter?

As observed by Foucault and others, a body in free rotation tends to preserve the "fixity of its plane" in proportion to the degree of force causing it to rotate. Thus, as shown in figure 3,

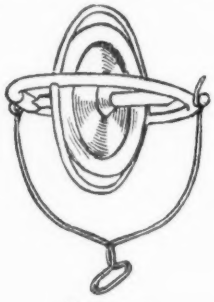

FIGURE 3,

a rotating disc supported underneath by a single wire, maintains its upright position till the force of its rotation is exhausted. This force, called by Thomson and Tait the "gyrostatic domination", tends to maintain the disc in one plane of

motion, out of which it is forced by the restraint of the arm resting on the pedestal, constantly pulling it away from the tangential direction in which the rotative force would impel it. That is, this *static* force, which tends to produce fixity of plane in the rotating disc, because of its tangential tendencies, resists the influence of the arm of the instrument, thereby causing the disc, instead of moving in a plane, to describe a cylinder or cone in its lateral motion. (Figs. 4 & 5).

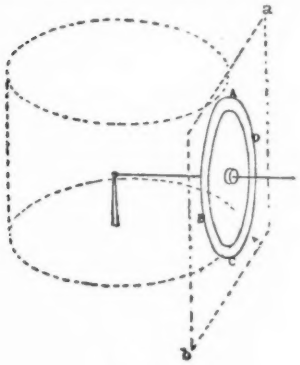

FIGURE 4.

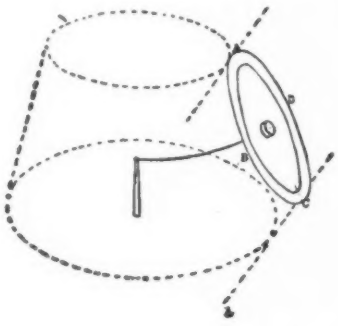

FIGURE 5.

Observing the instrument operating laterally (Fig. 6), and also looking at it from above performing its circuit around the pedestal (Fig. 7), we notice that the disc operates to sustain

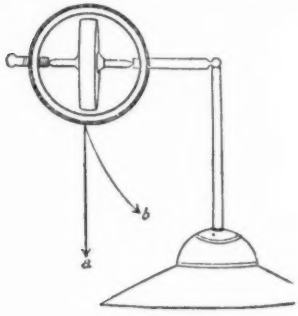

FIGURE 6.

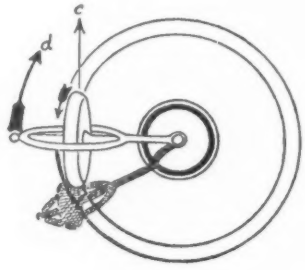

FIGURE 7.

itself on the one hand by this static force in its rotation tending to preserve its plane of motion in the vertical direction *a*, figure 6, and the tangential direction *c*, figure 7, and on the other hand by the restraint of the arm resting on the pedestal which constantly pulls it away from its tangential tendencies

both vertical and horizontal, so that it is balanced and upheld between them and this restraining arm. If the axial rotation of the disc lessens, the opposing power of the arm is relatively increased; and so, as the motion of the disc weakens, the instrument revolves faster around the pedestal. The action of these opposing forces, that of the disc in rotation to preserve the same plane, antagonized by the arm on the pedestal pulling it away from that plane, explains in a general way why the disc and its carriage are supported, but not fully so. The forces thus disclosed would affect nothing toward sustaining the instrument were the disc to remain in one position without movement to the right or left, because these tangential tendencies in opposition to the pull of the arm would not then be evinced. If the outer extremity of the supporting carriage is held on the fingers, it has no supporting power whatever, but falls as the hand is lowered. What then causes the gyroscope to revolve around the pedestal always in the same direction as the movement at the bottom of the disc and opposite to the movement at the top? As this movement is essential to the sustaining power, the answer to this question must also tend to show why the disc is supported in its revolution around the pedestal.

Certain early investigators, noting the fact that forces inducing rotation and its reactions act always outside of the center of gravity, and that the atmosphere grows denser the nearer the Earth is approached, assumed that after the disc is set in motion by a force outside its center of gravity, the disc reacts against the denser air at its lower part causing the instrument to rise.*

That it is not the influence of the atmosphere determining either the uplift or direction of revolution is shown in the fact that the instrument acts precisely the same under the receiver of an air-pump with the air exhausted as without; and that it is a force equal to the weight of the disc and carriage which determines the direction of this motion, is shown in the fact that if an arm sustaining the disc projects over the pedestal and a weight is placed upon it just balancing the opposite

* See Poisson's Mechanics, pp. 163-6—"On the Deviation of Projectiles; and On a Remarkable Phenomenon of Rotating Bodies," by G. Magnus. Memoirs of the Royal Academy of Berlin, 1852. Also Major J. G Barnard, A. M., "On the Motion of the Gyroscope as Modified by the Retarding Forces of Friction, and the Resistance of the Air: With a brief Analysis of the Top." American Journal of Education, 1857.

side, as shown in figure 8, revolution of the disc around the pedestal is prevented, the carriage remaining stationary; if

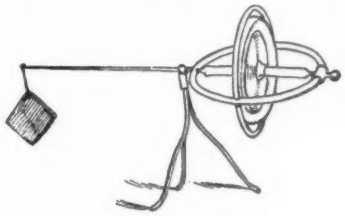

FIGURE 8.

however this weight is made to overbalance the opposite side, the disc and its carriage then revolve around the pedestal in the direction of the former's rotation at the top, thus showing that some force just equal to gravity determines this direction.

Shortly after Major Barnard published his explanation of the gyroscope in the *American Journal of Education* for 1857 ascribing the direction of this movement and increased motion at the bottom of the disc to "the retarding forces of friction and the resistance of the air", Hubert Anson Newton, then Professor of Mathematics in Yale College, published in the same journal an analysis showing that the force controlling the gyroscopic disk "must be a constant in the vertical plane in which the center of the disc moves", and finally that "it requires a constant downward force at the center of the disc to produce a uniform horizontal motion of the center of gravity." In this he anticipated, without explaining the action, the very result presented in the present paper, and for which he deserves prior credit. For just so surely as the law that any body acted upon by two forces not in the same straight line must take the direction of the diagonal of the parallelogram made by those forces, and its converse that the direction taken by a body acted on by two forces must be the diagonal of the parallelogram which those forces describe—just so surely as this law is correct, will it be found that gravity, as the chief force controlling gyroscopic action, is exerted as a push from above and not as a pull from below.

NECESSARY POSITION OF INSTANTANEOUS AXIS.

According to the principles of dynamics, the direction of this movement in the instrument (Fig. 1) must be the effect of a relatively increased motion at the bottom of the disc, and since there is nothing in the disc itself to increase the motion

at one part more than at another, this must be caused by some retarding influence at the top which must act from above the center of the disc and in the vertical plane in which the center of the disc moves; because the reaction of a rotating body from any external force, whether a push or a pull, is always in the direction of the movement on the opposite side and perpendicular to the direction of that force. As the gyroscopic disc always reacts at right angles to this perpendicular and in the opposite direction from what it would if affected by a force from below, the force effecting this result must be above the center of the disc.

As every axis is a center of least motion, so there is a tendency to form a new axis at the point on a rotating disc where any force operates to stop the motion. The new axis which such a force tends to form, is called the instantaneous or precessional axis—as all precessional movements are thus caused—and in the gyroscope the constant effort of the disc to move round such a point, determines the uplift as well as the direction of the revolutions around the point of support. As this is a necessary consideration is comprehending the instrument's action, let us notice what the effect of a force arresting the motion of the gyroscopic disk at any point would be, were it possible to locate an axis at will, in determining the direction of the movement which the disc would necessarily take.

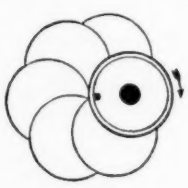

FIGURE 9.

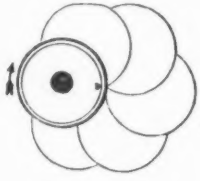

FIGURE 10.

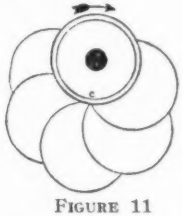

FIGURE 11

If such an axis were located at the left of the disc (Fig. 9,B), supposing its motion to be from left to right like the hands of a clock, its relative increase of motion on the opposite side would cause a downward movement. Though the disc would tend to make the circuit around the new axis as shown, yet as the initial movement must necessarily determine its direction, with the increased momentum helping to force it downward, it would move to the ground or plane of its support and stop.*

Were such an axis located at the right hand of the disc (Fig.

* See "Gyroscope" in American Cyclopedia, by Prof. Levi Reuben.

10, D) rotating as before, the effect of the increased motion on the opposite side would be to keep the disc constantly rising.

If such an axis were formed at the bottom of the disc (Fig. 11, C), it would necessarily move downward to the right or left in the direction corresponding to the rotative movement at the top.

Finally, if this axis is located at the top of the disc, the disc

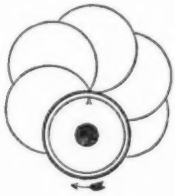

FIGURE 12.

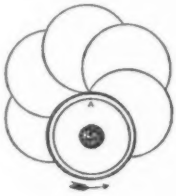

FIGURE 13.

will tend as a whole to swing around this axis in an upward direction because of the increased motion at the bottom. If the movement there is to the left (Fig. 12, A) it will move to the left; and if the motion there is to the right (Fig. 13, A) it will move to the right

FRICTION NOT THE CAUSE.

The downward pressure of the frame on the pivots of the disc might retard the motion on the upward side, and certain experiments seem to explain the movements in this way. Hold up in the hands an instrument as shown in figure 14, by two strings. It is apparent that if the instrument is held only by

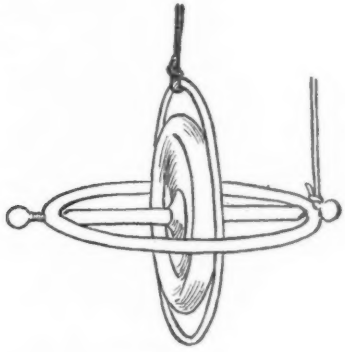

FIGURE 14.

the string attached to the middle of the frame, and the pivot ends of the axle enter sockets of the framework as shown in figure 6, that the weight of disc and axle will then rest on the frame and the greater friction will come from below; but if such an instrument is held merely by the string attached to the extremity, the instrument maintaining a horizontal plane in rotation, the weight of the frame will then rest on the axle and the greater friction will then be on the upper side. Set the disc in rotation and it will be found that its movements of revolution around the outer string will be reversed according as the instrument is supported chiefly by one string or the other. If supported chiefly by the inner string, the outer one merely acting as a stay and center of revolution, it will move in the direction of the motion at the top around the outer string; but if the weight of the instrument is thrown entirely on the outer string, the instrument reverses its circuit and moves around the outer string in the direction of the movement at the bottom of the disc. From such experiments one might come to believe as the writer once did that here is the true explanation of these movements. But suppose we have an instrument such as is actually shown in figure 14, where the ends of the axle are sockets and supported by pivots from the framework, then the bearings of the axle as related to the top and bottom being reversed, if friction of the pivots were the cause, the direction of revolution around the supporting center in each of these cases should be reversed. But such is not the case, the movements being precisely the same with both classes of instruments. Friction merely simulates here the true cause of the gyroscopic effects; so that one might erroneously suppose the rising and falling of the apparatus to be due to friction against the shaft as it moves upward or downward. Engineer James B. Eads once explained the gyroscope's action in this way in a published article, but subsequently told Dr. W. T. Harris, late U. S. Commissioner, that he had found his own explanation to be wrong and that he did not see how to correct it.*

Gravity's Static Effect.

The real cause has been overlooked doubtless because of disregarding the difference between the action of a rotating disc

* Proceedings Acad. of Science of St. Louis, March 10, 1856; also Proc. Phil. Soc. of Glasgow, 1857-8, articles by Edmund Hunt, C. E.; and "The Dynamics of Rotation" by Prof. Worthington.

free to move in every direction and one whose axis is fixed. This difference is quite as marked as that between the freedom and bondage of a human being, or indeed of the human mind itself when free to act, as contrasted with one enslaved in any way. When a rotating disc is perfectly free to respond, it must indicate in its action every force that affects it.

The fact is that gravity acts on one half the disc (Fig. 15) —the upward moving side—to retard the motion, and on the opposite or downward moving side to accelerate the motion, leaving the middle vertical line from the top through the center as the only direction where the action of gravity is constant and uniform. If the disc weighs ten ounces, it is clear

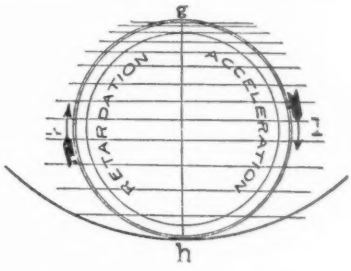

FIGURE 15.

that a force of five ounces is lagging back—retarding the movement—on the upward moving side, and another force of five ounces is assisting the movement on the downward moving side; while between the two, where the pull on the one side ceases and the push on the other begins, there is a vertical line where the force of gravity is uniform and constant.

On every fixed axis where the velocity is necessarily equalized on all sides, there is no opportunity for this peculiar effect to be shown; but in this case where the entire disc is free to respond, the constant influence of gravity in this vertical line as contrasted with the two sides is necessarily evinced. These retarding and accelerative effects offset each other upon the sides and the full force of rotation is left operative in excess of the influence of gravity in all parts of the disc except in this median line. The ratio which the energy of rotation bears to gravity is very great, sometimes 100 to 1; so that the contrast between the effects in this median line and other parts of the disc, is accordingly great.

Gravity being unaffected in this median line only—the retard-

ing and accelerative effects of the rotation balancing each other on the sides—it happens that this peculiar influence operates as a delicate clutch or penetrating plier exerting a staying power upon and through the entire disc; and it is a remarkable fact that there is no other known situation with such peculiar limitations, as is exhibited in this line or plane of the active gyroscope and top, where these upward and downward rotations meet, so as to disclose the true character of gravitation.

The strain of the two sides constantly exerted in the median line is necessarily greatest at some point along that line constituting the new or precessional axis determining the direction of the movement around the pedestal; and it is also evident that, so far as the revolutions are in the same direction as the rotations, the two movements will tend to overlap and coincide, the revolutions tending to quicken the rotations or rotative velocity of the particles. These two movements in the lower half of the disc being in the same direction, the rotative velocity of the particles there will tend to increase; and being in opposite directions in the upper half, the rotative velocity of the particles there will tend to decrease. The twisting strain exerted to equalize these tendencies, resulting from the rigidity of the disc and the force exerted at or between *g* and the center of gravity (Fig. 15), determining the direction of revolution, tends to bend the upper half of the disc back on its axle and the lower half outward thus disclosing the way in which the united weight of disc and carriage is thrown on the pedestal or supporting center. When the supporting arm or lug is made flexible (Fig. 2) this is clearly shown.

It is also evident that if the vertical line *g-h* (Fig. 15) represents the axis of revolution, the velocity of the particles of the disc in rotation will tend to increase or diminish according as they recede from or approach this line; that is, their velocity will tend to diminish from *k* to *g*, and from *l* to *h*, and to increase from *g* to *l* and from *h* to *k*. But these similar tendencies being opposite to each other, they will be seen to have, contrary to what some have held,* but little effect toward supporting the disc. A close observation of these

* See "Theorie des Kreisels", F. Klein und Sommerfeld, "The Gyroscope by S. Tolver Preston", in "Technics" for July 1904, and by Edwin Edser, in same Journal for Sept., 1904.

various influences, however, shows that, if the disc were not influenced by an instantaneous axis at the top, but at the bottom instead, or, if it merely descended without the formation of any such axis, its movement around the pedestal would be in the opposite direction from what it really is. The tendency to an increased velocity on the downward moving side *g-l-h*, combined with the tendency to increase the velocity of the particles in the upper quadrant of that side from *g* to *l*, would result in forcing the disc's revolutions in the same direction as its rotation at the top.

Professor S. P. Langley, shortly before his death, told me that an expert, highly recommended, whom he employed to determine the mathematics of the gyroscope, reached this very result—that is, the instrument revolved in the opposite direction from which his analysis determined that it should. There is little doubt that Poisson laid the foundation for this error in his original paper on the gyroscope. A pupil of a distinguished Cambridge professor once witnessed a similar mistake on the part of his instructor; and so eminent a scientist as Tissandier presents an illustration in his "Scientific Recreations" where the gyroscopic disc, according to the arrows and description, revolves in the same direction as the rotation at the top—the direction it would take if gravity's effect were greatest at the bottom, but an impossibility in fact!

That the effect of gravity is greater at the top than at the bottom of this median line is apparent from the movement, but why is it so? It may be said that the influence of gravity along this line being least at the bottom, where it affects very little of the disc, and greatest at the top where it affects the whole diameter, the motion is necessarily checked most at the top and the resultant velocity greatest at the bottom. But the truth is that *the gyroscope experimentally demonstrates in this action what nearly every scientific man believes, that gravity is a pressure exerted at every point in a direct line from above toward the center of the Earth, and not an attraction from below*; since if it were an attraction the effect of gravity would be the greater at the bottom, the disc would revolve in the opposite direction and speedily be brought to the ground.

Reaction of the Rotative Energy.

Gravity, however, acting with greatest force at the top, action and reaction being equal and opposite, *that proportion of the*

sustaining energy involved in the rotation just equal to the weight of disc and carriage, centers at the top in its reaction against the natural push of their united weight downward. Accordingly when this weight is balanced by an equal weight on an extension of the arm over the pedestal (Fig. 8), the instrument ceases to revolve; and if slightly overbalanced, it revolves in the direction of the rotation at the top of the disc. For the same reason when a downward push is exerted on the frame increasing the reaction, this pressure is resisted, and the instrument rises as the pressure is taken off. This instantaneous check which gravity exerts at the top, tends to throw the entire disc around this point as a new axis in an upward direction. (Figs. 12 & 13, and *g*, Fig. 15).

The resulting movement is shown in figure 16, where A represents the outer side of the disc, A′ the position to which gravity would naturally cause the disc to descend, and A″ the position to which the check on the instantaneous axis at the top and greater accelerative acting at the bottom tend to impel it. Between the two it moves in the direction of its motion, M-N being a section of its circular path of revolution around the pedestal.

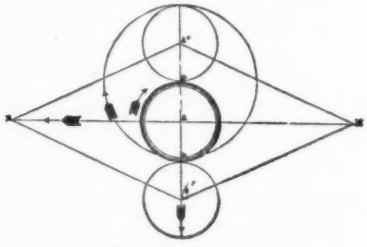

FIGURE 16.

THE COMBINED FORCE.

The gyroscope is upheld and its entire weight thrown on the pedestal then as the resultant of two sets of opposing forces (Fig. 17), first, the rotation of the disc tending to preserve its plane of motion—in effect pulling away from the pedestal, opposed to the arm in effect pulling toward it; second, gravity naturally causing the disc and carriage to descend, as opposed to its static effect on the disc checking the motion at the top and accelerating it at the bottom, thus tending to throw the disc upward. These forces in their effects unite in the vertical line joining the top and bottom of the disc, which in the first pair is constantly in the plane which the disc's axial rotation

tends to produce, as well as in its path of revolution around the pedestal, and in the second pair forms the radius of the

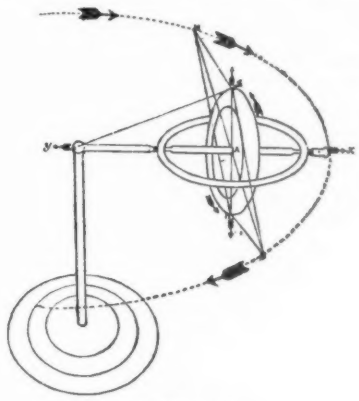

FIGURE 17.

new axis about which all these forces center, tending to force the disc upward.

Why this effect of gravity on the disc is not disclosed in a rotating body whose axis is unalterably fixed and the body thus rendered incapable of free motion, is easily explained. Then the tendency to form this instantaneous axis along with the forces of the two sides, one retarding and the other accelerating the motion, are all equalized at the top and bottom as well as on the sides, in every part of the disc's rotary movement. Practically, of course, in the free moving gyroscope, the velocity is the same at the top of the disc as on the sides, but the strain due to the inequality of the forces on the two sides is exerted as a stress centering in the median line, where the force of gravity is greatest and constant, determining the movement of the disc as a whole.

A body must have opportunity before it can offer resistance; this is illustrated by various experiments. A toy buzz offers such resistance when we attempt to change its plane of motion by suddenly loosening up and altering the direction of the string, because it then has momentarily free movement like the gyroscopic disc. Also the common top resists alteration of its plane of motion in the same way; and by reason of the retarding force of gravity being greatest on its upper surface producing a new or precessional axis there, it regains its equilibrium when it careens. But fly-wheels and all rotating bodies,

whose axes are fixed and carriages incapable of free movement, exhibit no differences as to the retarding or accelerative force of gravity, because there is no opportunity for the necessary reactions to occur. But if for any reason play is afforded to the axles, such bodies may burst at a comparatively low rate of velocity because of the unequal stress induced by gravity. As already referred to, if the outer extremity of the carriage of the gyroscope is held in the hand, however great the velocity, it does not rise from the hand and has no supporting force whatever, because no tangential force is exerted, the freedom of the disc's motion being destroyed; and such a result may be made periodic, alternative and reciprocal, as illustrated in the following experiment:

Suspend by a cord a disc mounted in one on more extra rings (Fig. 18), so as to afford at least three directions of motion.

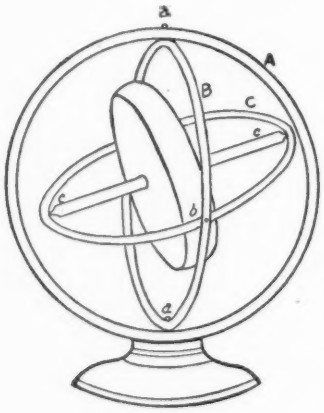

FIGURE 18.

The disc set in rotation will tend to preserve its fixity of plane in swinging it in a circle or in any direction; but by grasping the outer rings with both hands, while the disc is rotating, and giving the apparatus a slightly twisting and oscillatory motion so as to cause this static action of gravity to vary its effects, that is, periodically destroying the disc's freedom of movement, it may be made to whirl in either direction with considerable speed and at right angles to the plane of its movement in rotation, thus showing this peculiar influence of gravity to be a chief factor in supporting gyroscopic action of every kind.

SUMMARY.

Three chief factors enter into the problem of sustaining the common gyroscopic disc in its circuit:

1—The rotation of the disc representing the applied energy, tending to preserve its plane of rotation tangentially to the point of support.

2—The pressure of gravity resulting from the inequality of movement in the two sides forming a precessional axis above the center in the median line, thereby tending to throw the lower part of the disc upward in the direction of its rotation.

3—The reaction of the rotative energy equal to or exceeding the weight of disc and carriage, thereby throwing the weight back on the sustaining arm and pedestal.

THE ORIGIN OF THE LUNAR SURFACE FORMATIONS.*

WALTER GOODACRE, F. R. A. S.

There are, speaking generally, two theories held to account for the existence of the so-called craters on the Moon's surface. The one ascribes these to volcanic energy acting from within, and the other to forces external to the Moon, or in other words to the impact of foreign bodies on its surface when it was in a plastic condition. This latter view was held for a short time by Proctor and to a modified extent by the late geologist Professor Shaler, who ascribed the formation of the lunar Maria to the impact of bolides from 10 to 15 miles in diameter. It has also been advocated by others, including Mr. R. Parry, one of the Members of this Association.

The latest supporter of the "impact" theory is Dr. See, the well-known American astronomer, who, in an article in the 1910 February number of the Publication of the Astronomical Society of the Pacific, contends that the lunar craters and Maria great and small were caused by the impact of satellites. This impact theory has been so little countenanced in the past by selenographers that when we find it advocated by an emi-

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nent astronomer like Dr. See we are bound to carefully examine the grounds on which he claims our adhesion to his views.

In his opening remarks Dr. See refers the reader to a paper in No. 4367, 1909, November,of the *Astronomische Nachrichten*, where he states he has treated of the problem of the obliquities of the planets from the point of view of the capture theory, and has shown that "Jupiter's small obliquity has been caused by the capture and absorption by that giant planet of a vast quantity of satellites moving about the Sun in planes nearly coinciding with the plane of the planet's orbit. It has been shown by calculation that if the mass of Saturn were increased by this process of capture till it became equal to that of Jupiter, the obliquity of Saturn would become as small as that of Jupiter, whence it is inferred that Jupiter's obliquity was once large and afterwards gradually destroyed by the growth of his mass from the capture of satellites moving near the plane of his orbit. This theory of the planetary obliquities is applied to other planets of the solar system, and the facts are shown to be in good agreement with the theory."

This theory of accretion is very interesting and ingenious, but it has yet to meet with the general acceptance of astronomers. In looking about for evidence in support of his views, Dr. See cites the present appearance of the Moon's surface as a case in point, and it is in connection with this contention that what follows has been written.

It is, I think, important at the outset to state that, so far as I am aware, no selenographer of repute has ever held similar views to those set forth by Dr. See, though he does mention Schmidt as one such.

Schmidt's support, however, simply amounts to this: that "he became doubtful of the volcanic origin of the circular mountains mainly because the inner parts of the craters were found to be depressions with central peaks below the average level of the lunar surface."

It would be interesting to hear if Schmidt had any alternative explanation to account for the origin of these objects, but on this point Dr. See does not enlighten us.

It is significant that those astronomers who have become familiar with the details of the lunar surface through years of close study with adequate telescopic assistance, one and all ascribe the present condition of the lunar surface to the effects of volcanic agency and find in that force a sufficient cause to produce the results now apparent.

Professor See sets forth his arguments in a series of 24 paragraphs, which are so concisely expressed that it is difficult to compress any one of them into fewer sentences than he uses.

Space and time will not permit me to deal with all his statements seriatum, so that those who are interested in this matter and wish to look into Dr. See's views more closely should consult his article, which is now in our library.

Dr. See's first statement is to the effect that the impact theory explains the following facts:

"Both large and small craters and their superposition over one another, some being older and others newer as the case may be."

In reply to this, it may be briefly asserted that the volcanic theory supplies an equally acceptable explanation of these appearances. Further, it is a curious fact that these superimposed craters are all, so far as I know, smaller than the crater on whose area they have intruded themselves; they also bear the impress of being of a later date. This is quite as we should expect if we adopt the volcanic theory, as the superimposed craters would be formed subsequent to the larger ones and at a time when volcanic activity on the Moon was feebler.

If we adopt Dr. See's view, we must pre-suppose a period when large satellites abounded and a later period when only smaller satellites were left to impinge on the Moon's surface.

Dr. See finds it difficult to account for the small craters on the rims of the larger by the theory of eruption. Now the number of craterlets found actually on the rims of the large craters is comparatively small or not so numerous as Dr. See thinks; there are, however, numerous craterlets associated with the large ring-plains, but these are found on the outer slopes or at the foot of the slope on the inner side. In this respect we can fortunately find terrestrial parallels.

The volcano Etna is a case in point. In the recent eruption it was stated that large numbers of craters were formed on the outer slopes of the main crater. These craterlets on the Moon are often found in chains running round the inner or outer slopes of the large craters and when in some cases they have become confluent a rugged valley or crevass has been formed.

Dr. See's third argument in favor of his theory reads as follows: "The existence of craters in perfectly smooth plains as well as in the rough and broken regions, and the unequal density of the craters in the different parts of the lunar surface.

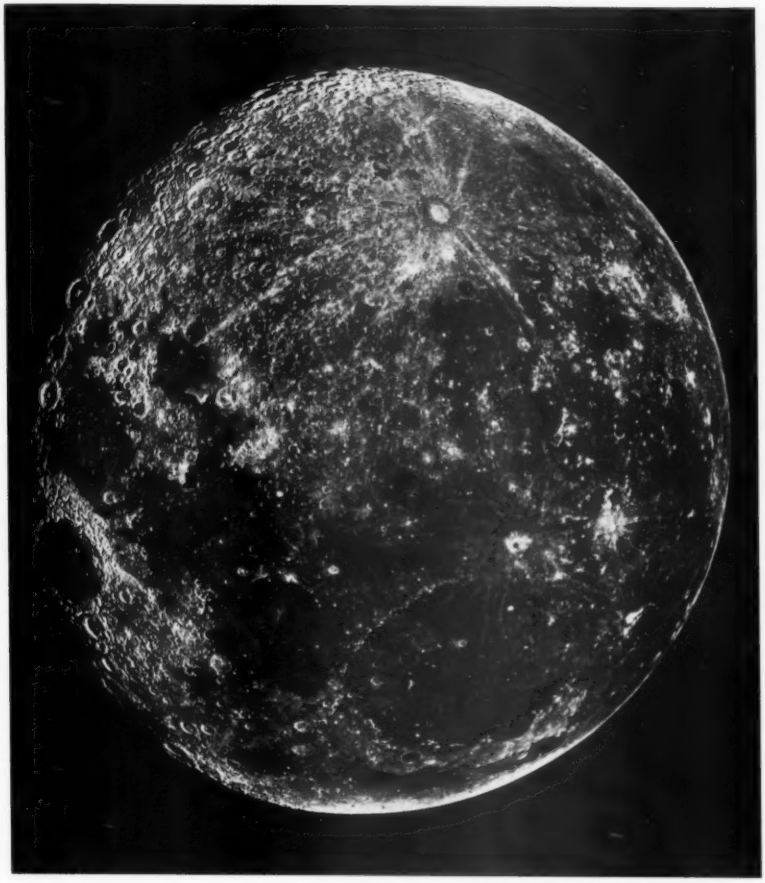
Terrestrial volcanoes generally follow the mountain ranges along the sea coast, on the Moon the craters are scattered indiscriminately except that they are rare on the Maria for reasons which will hereafter appear."

These statements correctly describe the distribution of the craters on the lunar surface, and this condition of things is just what we might expect as the result of volcanic energy.

The reason there are comparatively so few craters on the lunar plains is, if we accept the volcanic theory, explicable on the assumption that the Maria were formed during a very active period of eruptive energy, and after the surface had solidified the internal energy left was too feeble to form anything but the smallest of these objects.

There are some facts in connection with the craterlets on the Moon which make it difficult to conceive that they could possibly be formed by the impact of satellites; I allude to the large number of cases where craterlets are found in chains with their walls in contact. It seems impossible to imagine that foreign bodies of any sort falling on the Moon should so arrange themselves as to form straight or curved lines; this view becomes more difficult still when we find that in some of these chains the leading crater of considerable size, and those which follow show a gradual and regular reduction in size. Several illustrations might be given, but I will mention only one. Outside the W. wall of Ptolemaus is an irregular crater ring marked *r* in Neison's map and *b* in Schmidt's; it is about 12 miles in diameter and has a pair of smaller craters abutting on its southern rim, which has been deformed by them. These two craters are about five miles in diameter and N. E. from these runs a chain of craterlets as far as Ptolemaus G. This chain consists of six or seven craterlets which, starting at about four miles in diameter, gradually become smaller in size until the last one does not exceed one mile in diameter. Dr. See would have us believe that six or seven satellites reached the Moon in a straight line, at the head of which was the largest and at the opposite end the smallest. It seems much more reasonable and in accordance with appearances to suppose that the largest crater was at the focus of volcanic energy and that the smaller craters represent a steady decrease in the energy as the distance from the center increased. Several more instances of craterlets arranged in straight or curved lines can be found with sufficient telescopic power on the plain N. of Stadius as well as in other districts. Another

PLATE IX.



THE MOON.

Photographed by MM. Loewy and Puiseux

Paris Observatory, Sept. 2, 1898.

statement that Professor See makes is that "The greater steepness of the inner walls and the great diameter of the large craters which could not be well explained by volcanic forces."

In reply to this it may be noted that in all terrestrial craters the inner walls are steeper than the outer, and the difficulty of the greater size of the lunar craters as compared with terrestrial craters may be explained, if we remember that gravity, at the Moon's surface is only one-sixth of that on the Earth's, and if we may assume that volcanic energy had the same power in both cases, then we should naturally expect to see greater results on the Moon than on the Earth; this view is strengthened if we allow, which is probable, that the average density of the materials forming the outer portions of the Moon's surface is less than those forming the outer portion of the Earth's crust.

It has been previously pointed out that if the lunar craters were formed by the impact of satellites, these would reach the Moon's surface at various angles, and this would have the effect of making the indentations vary in shape; some would be circular, others elliptical, and when the angle was very acute a long furrow would be formed. Dr. See meets this objection by saying, "If it be thought that more large craters should be elliptical than are observed, it may be recalled that even if the first contact with the Moon produced such an outline, the impact of a large satellite would generate enough heat and underlying flow to force out the walls about symmetrically all around, and the final figure would be circular like the globular figure of the satellite. Thus craters which are, say, ten times as wide as they are deep, ought to be almost circular, whilst smaller craters would be more irregular and elliptical as found by observation. This is because the forcing out of the material beneath small craters is less effective than in the case of large craters, and they retain more nearly their original form of first contact." Now it appears to me that observation shows conditions the exact opposite of this. We find that the largest rings depart most from the circular form, and that the smaller ones are the most symmetrical; indeed, the smaller the crater the more does its shape approach that of a perfect circle. This is precisely what we should expect as the result of volcanic action; it is, however, only fair to mention in this connection that the large rings may have originally been more circular than at present, and that their symmetry has been destroyed by the effects of the shrinking of the Moon's crust

due to cooling, and this has dislocated the rings, in many cases causing them to assume the polygonal form with which all lunar observers are so familiar.

Dr. See's next reference in support of his theory is concerned with those interesting features of the lunar surface, the ruined and obscure ring formations. He says, "The very flat-bottomed craters noticed in such regions as the Mare Nubium are due to the filling up of the deeper and more irregular craters with cosmical dust, or by melted material which has assumed a level surface; this has at length become so deep as to leave only the walls visible about a level central area, whilst the central peaks have been nearly or entirely covered up . . . so far as one can see two, and two only, explanations are possible: (1) The deposit of cosmical dust from the heavens and from conflagrations arising in the impact of satellites; (2) The partial melting down of the walls by the conflagrations which produced the Maria, so that only an outline of the original crater can be traced."

That cosmical dust falls on the Moon as on the Earth is, of course, admitted, but that it should have fallen in such quantities as to have produced the results claimed for it by Dr. See seems open to doubt.

Again, if cosmical dust has fallen in such large quantities as is suggested, why was it deposited on the Maria more than on other regions; if it had fallen equally over the surface, then surely we should have evidence of the partial filling up of the deep craters, which is not apparent; and, further, would not this vast amount of cosmical dust have filled up the numerous clefts and blotted out the bright rays?

That the lunar seas have been formed by the action of molten lava outwelling from the interior is generally admitted, as many objects on the surface of these plains as well as on their margins bear evidence of having been subject to some erosive force. The point in dispute is as to the means by which this lava reached the surface. Dr. See thinks it was the result of the crust being fractured by the impact of satellites, others think that the lava issued from fractures in the crust caused by subsidences through the cooling and shrinking of the interior matter. Tidal action may also have had some part in bringing about these outbursts. If we think with Dr. See, then we must admit the existence of very large bodies impacting themselves on the Moon to have caused the overflowing and levelling of such vast areas. The Mare Crisium, one of the smaller of

the lunar seas, is over 300 miles in diameter; if this originated as the result of the impact of a single satellite its size must have been very considerable, in which case the results to the Moon generally would probably have been more serious than appears to have been the case.

The origin and nature of the bright rays on the Moon's surface have never been satisfactorily explained, but according to Dr. See his impact theory provides the solution of this difficult problem. He says, "The shorter streaks from such craters as Copernicus and Aristarchus are easily explained. It is sufficient to suppose that the collision was so forceful that matter was scattered out in all directions, and, perhaps, heated to fusion in the process, yet as the Moon has no oxygen it did not burn and blacken as meteoric stones do in falling on the Earth, but took on a fused and glassy aspect which by reflection gives the brightness of the shorter streaks radiating from Tycho and its associates. This explanation was given many years ago by Mr. Würdemann, of Washington, D. C., in a letter to Dr. B. A. Gould, but it seems to be little known to astronomers." In connection with this statement, the argument is weakened when we remember that the bright rays are not continuous lines of light extending over vast distances but are broken up into sections, and in many cases, as pointed out by Professor W. H. Pickering, they commence or terminate at a small bright crater, and it is probable that they are cracks out of which bright colored material has been ejected to fall on either side, or it is quite possible they may be deposits of salt, as suggested by Mr. H. G. Tomkins. Further, these rays do not always radiate from the large crater with which they are associated, as is shown in the case of Tycho, where the focus of the rays is outside the ring. The weakest link in the chain of Dr. See's argument seems to me to be that in which he ascribes the formation of the lunar clefts to the impact of satellites. He remarks, "The clefts are cut by glancing satellites, which thus leave a straight or curved line according to the nature of the surface and the resistance and rebound. Photographs confirm this origin of the clefts, and show that they are not cracks but actual cuts, sometimes more than 100 miles in length."

Photographs may appear to lend some support to this theory, but it is difficult to conceive a glancing satellite cutting its way like a plowshare for many miles, diving under mountains and reappearing on the other side or curving and twisting

about as some clefts do. Such a grooving process would result in the displaced material being piled up on either side just as Dr. See says the mountain rings have been formed by the impact of satellites, but the examination of the edges of the lunar clefts with adequate telescopic power does not disclose any such banks as might be expected. It is when the telescope is directed to the examination of these objects that the weakness of Dr. See's argument becomes apparent. It will be found that these clefts are not all alike, but may be divided into several classes; a smaller number are undoubtedly of volcanic origin and consist of craterlets in chains whose separating walls have disappeared. The greater part of the Hyginus cleft appears to have at one time consisted of a chain of inoculating craters, and I have detected evidences of confluent craters, in parts of the Ariadæus cleft. The greater number of clefts on the Moon, however, appear to be simple cracks and caused by the shrinking of the surface at the time of solidification; as examples of these the intricate branching system of clefts on the west side of Triesnecker may be mentioned, as well as those found on the interior of such ring-plains as Cleomedes, Posidonius, and Gassendi.

It is also a significant fact that we find the cracks in exactly the places where we should expect them, as a result of shrinkage; namely, on the margins of the great plains or seas. One or two notable illustrations in this connection may be mentioned. There is a system of clefts, situated on the eastern side of the Mare Tranquillitatis, running from Guttemburg, past Sabine to Maclear. There is also another system at the western margin of the Mare Serenitatis, which commences on the interior of Posidonius, runs round the west, south, and east sides of the Mare, and under the lee of the Hæmus mountains beyond Sulpicius Gallus.

A third, the most remarkable of this series, is that on the west side of the Mare Humorum, and consists of three curved parallel clefts running concentrically with the border of the sea. Some other clefts, though not so important, are to be found on the eastern margin, extending from Doppelmayer to Gassendi.

That all these are cracks in the surface, and not grooves cut by glancing satellites, is so obvious when telescopically examined or theoretically considered that the argument need be labored no further,

Dr. See illustrates his article by three photographs, one of

the Moon near first quarter; another showing the effects of bullets fired into a leaden target; and a third showing the imprint of raindrops on a piece of rock made when, presumably, it was in a plastic state.

The two latter, Dr. See thinks, supply valuable evidence in favor of the impact theory, and of these he writes as follows:—

"If we compare the lunar photograph with the accompanying imprints made by raindrops and by bullets fired into a leaden disk as a target, we shall notice the most remarkable similarity in the two effects. The raindrops are, however, all fluid, and leave only saucer-shaped imprints and no central peaks, whereas the bullets and stony satellites indenting the lunar surface would necessarily leave central peaks in accordance with observation."

In respect of the appearances made by the bullets in the leaden target it may be admitted that at the first glance they have a striking resemblance to lunar craters, but the central peaks upon which Dr. See lays stress, and which are necessary to complete the analogy, are not there.

This reminds me of a photograph I once saw, which was submitted in support of a similar theory of impact, illustrating the effects of shells fired into an iron target, and the indentations in some cases resembled lunar craters, but the most striking resemblances were at the back of the target where the shells were seen to be emerging, and this I pointed out at the time as in no wise supporting the impact theory.

Those who have followed at all closely this interesting study of the causes of the lunar craters will have noticed several attempts to produce by artificial means objects resembling lunar formations, and which in some cases have been remarkably lifelike, but so far as my memory serves, one and all were produced by some force operating from the underside of the material used, and not by anything impinging from without.

Dr. See is so sure of the soundness of his arguments that he ventures to conclude the matter in the following somewhat dogmatic fashion.

"Accordingly, we may dismiss the old volcanic theory once for all as false and misleading, and may look upon our satellite as a battered planet, which presents to us the most lasting evidence of the process of capture and accretion by which the heavenly bodies were formed. The strength of the present argument regarding the origin of the lunar craters does not rest on one class of phenomena alone, but on several distinct

classes of phenomena, which are all harmonized among themselves, and brought into accord with the necessary processes of planetary growth. Since worlds form in nebulæ, it follows that impacts will necessarily occur, and the Moon's face shows the size of these masses by their imprints, which thus throws an unexpected light upon the state of the solar system in the remote past."

It only remains, so far as we are concerned, to see how far these arguments are in accord with facts and the present condition of the lunar surface; it is not necessary that we who stand by the volcanic theory should be able to demolish every one of the arguments that Dr. See puts forward. It will be sufficient if in one or two instances only we can show that his theory is untenable and contradicted by facts.

The effect of reading Dr. See's article, so far as I am concerned, has been not to shake my faith in the volcanic theory, but rather to confirm it as being the most probable cause of the origin of the lunar surface formations.

LONG-RANGE WEATHER FORECASTING AND ITS METHODS.

J. S. RICARD.

Hitherto the long range forecaster has been denied a seat in the banquet hall of science; the common people have distrusted him; the educated have shunned or thrust him aside; the general scientist has denounced him; the professional weather-man has treated him with supercilious scorn and has compared him to astrologers and cheap almanac-makers.

What the reason for this discouraging attitude may have been, it is not our purpose to inquire into. Let us rather pause a while and reflect that as alchemy gave us chemistry and astrology preluded the sublime astronomy of to day, so the noble efforts of those who have braved the jeers and sneers of their fellow-men in the pursuit of an apparently chimerical achievement may eventuate into a science that future generations shall be proud of.

But we fear the "may" is too modest; the proper phrase

should be "those efforts have given us the science of long-range forecasting," the philosophy of which is not far distant may be already at our door.

Some three years ago, an eminent meteorological writer said, "The weather can be assimilated to the phenomenon of water boiling in a cauldron, with a little float put into it; the behavior of the float will indicate the march of the cyclones and anticyclones across the Earth's surface." At that time, we gave expression to an emphatic *No*. That writer was missing the point at issue, viz., *what* does make the water boil, and next, *where* will the little float be at such or such a date, say March 25, 1911?

The prevailing idea at that time and even now is that the atmosphere is alone responsible for its own changes, and if aught else there be, it has so far eluded the grasp of science. The Weather Bureau of the United States does not appear to be much in love with the various ocean currents, and who can blame it for that? This idea does not bear scrutiny. We are placed in the solar system. Every system is full of mutual actions and reactions. The weather must be the result of an action or reaction or perhaps both. Which is which remains to be seen; but the thing concerns the forecaster less than the philosopher.

This however, seems certain, that by calculating the resultant of the electro-magnetic activity of the various members of the solar system both on the Sun and the Earth for a certain date, we can foretell long in advance that there shall be a disturbance on the Sun and a corresponding disturbance on the Earth; and, what is more, we can fix very approximately both the degree of intensity and the heliographical and geographical positions of those correlated disturbances.

Such, in essence, is the forecasting system of W. T. Foster of Washington, D. C., it resting on the physical basis of the accumulated observations of Weather Bureau men and the correlations of observed phenomena to planetary positions. To one accustomed by long training to the generalizations of science, the *prima facie* impression of the system is not only favorable, but seems eminently philosophical and in full accord with the present drift of the human mind to refer most phenomena of nature to the secret agency of electro-magnetism.

But is it true, and how does it work in practice? If we may suppose that, in meteorology, the criterion of truth is *success*, then we have to answer both questions in the affirmative. But

right here, some one may desire to know how long an idea is to be watched before we can tell about its truth or falsehood. The answer is that, by the canons of logic, an induction is to be considered valid, as soon as the invariability of sequence or concomitance makes it clear that the phenomenon is not accidental.

In our case, where the events under observation occur and recur $^{3}6^{5}_{8} = 121$ times or something like it in the course of the year, it would seem unreasonable to postulate many years. A European astronomer, a member of the fourth Conference of the International Union for Solar Research, told the writer that two or three years might suffice. But we have much more than that; Foster's experiences have run nearly one-third of a century and ours nearly one-tenth of a century.

Another thing that seems equally certain is that, while a solar disturbance is passing across the solar disc, a corresponding atmospheric disturbance is passing across the Earth's surface, therefore, observations can fix the date when a cyclone shall reach, say the Pacific Coast, just as well as it can fix the date when the solar cyclone shall reach a certain meridian. It belongs to this observatory to have discovered that when a solar phenomenon, spot, faculae, or both combined, reached a position which is within an average of three days from the western limb, a cyclonic area enters on the Pacific Coast. Cyclonic areas do also, but seldom and by way of exception, reach our coast when a solar event of the kind above described is yet *near* the eastern limb and when it is *near* or *on* the solar axis. And that is why, once in a rare while, we can have continued rains for 12 or 15 days and a recurrence for 12 or 15 days more. What the reason is for this extremely rare exception, we have not yet been able to find.

Another theorem of equal moment in the development of long-range forecasting is that, when a solar disturbance passes off behind the western limb, the anticyclonic area which always presses behind the cyclonic steps on the Pacific Coast, causes a few flurries and quickly brings on fine weather. On this theorem, we announced a great clearing for February 6, 1911, long before it occurred. Complications and modifications are bound to happen once in a while but these always correspond to like complications and modifications in the events across the solar disc. The substance, however, of the theorem above enunciated remains unchanged.

This whole meteorological doctrine looks very fine, so fine

indeed that in the eyes of some people it passes for hallucination pure and simple. People disbelieve, criticize, invoke principle, make a very riot of ratiocination. Let it be so! But the fact remains that aero-physics is not learned by disputation, but by observation, and we have to stand by the valid inductions of observation. The system rests on the physical basis of a long-continued study of the U. S. Weather map and the correlation of the recorded phenomena to the positions of solar phenomena.

It thus becomes evident that the science of prediction turns out to be a science of *positions* as it ought to be, just as the weather map is a mere record of positions, barometric and thermic.

In order to satisfy the reader that guessing at the weather is a thing of the past and that the stage of true scientific intuitions has been reached, we subjoin the following columns of storms recorded by the Weather Bureau and the long-range forecast issued by Foster and this observatory in regard to the Pacific Coast for the very critical and troublesome period, Jan. 8 to Feb. 6, 1911.

The Weather Map	Foster's	Ricard's
Jan. 8—10	Jan. 7—10	Jan. 8—11
" 11—16	" 12—15	" 12—15
" 16—18	" 17—20	" 17—20
" 18—21	" 22—25	" 22—25
" 23—26	" 29—Feb. 1	" 29—Feb. 1
" 27—Feb. 1	Feb. 1—4.	Feb. 3—6.
Feb. 3-5.		

Foster's forecasts were at least one month ahead of time; ours, at the most, two weeks ahead of time. The general identity of dates obtained on the planetary and the solar systems would seem to indicate plagiarism on our part. But not so. We profess absolute honesty in this matter. Let one instance serve for all. On February 8, after looking a few seconds at the Sun and discovering a body of faculae yet near the eastern limb, we immediately announced a new disturbance on the coast for February 15-18, while Foster's announcement reached here only on Feb. 9 for a disturbance within exactly the same dates, 15 and 18.

On the whole, however, whatever may be the merit of the solar method, we must confess that we regard Foster as the king of the world's forecasters and his system as containing the *rationale* of our own. We simply wanted to stop at the Sun for some time with a view to settle a difficulty of long

standing—astronomers having been so far extremely divided on the point of solar influence on the weather—and read some tangible formula in the affirmative or the negative. This result having been accomplished, we humbly present it to the learned, and unlearned as well, to do with it what they please.

Meanwhile and henceforth, we willingly cast our lot with Foster in the search after the cause of solar upheavals, which he says is to be found in planetary electro-magnetic influence. If solar phenomena can be foretold before they reach the eastern limb of the Sun or before they happen anywhere at all on the solar disc, by using the planets as the supposed factors, the trend of mind will be to make them responsible for the causation and proof can later on be rendered absolutely demonstrative by some sort of eliminative process. True, the correspondence of phenomena is no proof of causation; it may be mere concomitance; but the concomitance is sure to lead to the true cause. So far, all the predictions of solar disturbance made by Foster and addressed to us for verification have proved correct and we shall wait for more.

In keeping with his electro-magnetic view, Foster affirms that atmospheric disturbances come to us from one or another of the four magnetic poles or the magnetic equator. Weather Bureau records show that they reach our coast from the Alaska side or the valley of the Colorado, very seldom centrally. This observatory has shown that events north of the Solar equator correspond to our southern disturbances and, conversely, events south of the same equator to northern disturbances. To the question, why some events on the Sun affect our telegraphs and others do not, we answer that depends on their intensity, not their size.

Observatory, Santa Clara College,
Santa Clara, Calif. Feb. 14, 1911.

VEGETAL LIFE ON MARS.

LATIMER J. WILSON.

An observation of the surface markings of a planet is entertaining and profitable to the layman who possesses a moderate sized, or even a small telescope. To peer into the more remote depths of the sidereal universe and to record the scant evidence of change in such distant regions requires the best scien-

tific equipment. Large telescopes arranged for photography are far in advance in this field of research, but unfortunately they are usually beyond the means of the ordinary layman interested in astronomy. The precision attained by photography in the study of the stars and the nebulae is in many ways unsurpassed by visual methods.

Conditions are somewhat different, however, in the study of planetary details. Here is less difference in the efficiency of good telescopes, large or small. While it is true that to show the finest details on a planet, a large or moderately large instrument, mounted in a favorable place, is required, much valuable work has been done with modest means. One has only to become acquainted with the work of such able men as Beer and Mädler to see what can be done with a small telescope.

Many who do not particularly study the heavens were attracted by the brilliant red light of a planet which ornamented the sky during the autumn evenings of 1909. The planet was Mars, which on November 16 will again be comparatively near the Earth. Its distance will then be about 47,500,000 miles, or about 11,000,000 miles farther from us than in September, 1909. This planet, of all the members of the Solar system, the Moon only excepted, probably affords the greatest pleasure to the possessor of an efficient telescope. Whether or not the planet is an inhabited world, its physical aspect sufficiently resembles the Earth to make it an object of great interest. The views of its surface obtained with the telescope are sure to awaken the imagination of a thoughtful observer. There is a beauty in the changing panorama of the Martian map which is not to be forgotten by one who has patiently studied its features.

Seen for the first time under favorable conditions, the planet suggests the naked eye view of the gibbous or full Moon. A brief scrutiny discloses many points of dissimilitude. If its position with relation to the Earth is such that one or both of the white polar caps can be seen, the observer will at once recognize a world quite different from the Moon. A sufficient study of the polar caps alone would disclose the fact that Mars undergoes changes of regular recurrence and that it is therefore a living world. Another point which distinguishes Mars from the other planets of the solar system is the striking color of its disc, the greater part of which is of various tones of yellow, from reddish yellow to a dull, brass color. These vast

regions are probably deserts, and it is the sunlight reflected from them that imparts to Mars its ruddy color, and distinguishes it as the red planet.

The irregular band of dark markings which gird the planet in the southern hemisphere, when first seen, appears to be gray or brownish, or about the same color as the dark regions of the Moon. A more careful inspection shows them to possess many varieties of tint; emerald, sea-green, dark blue-green and indigo are the most noticeable. The blue-green areas were at first thought to be bodies of water and were accordingly so named on the maps. Though their names as such are retained they are now thought to be vegetal areas, occupying the lowest levels of ancient seabeds. In the greater part they undergo changes of tint which correspond to the seasons of the planet's year. From pale green in the spring, to a darker blue-green in the summer, they fade in the autumn—a characteristic with which we are familiar in the plant life of the Earth. Their color actually suggests that of our own verdant landscape as seen from a distance. Of course we cannot see the ultimate surface of Mars, but only the blended light from comparatively large sections of the surface. It is significant that the darkest of the blue-green sections and those less subject to change, are in the tropics, and the darkest part of these generally border the great deserts which lie north of them. The warm air over the tropical desert of Mars would absorb moisture from the surface, and probably, as on the Earth, the greatest precipitation of moisture would occur in the vegetal areas near the equator. The apparent density of the vegetation on the side toward the tropical deserts might thus be explained.

The physical aspect of Mars is rather anomalous when compared with the picture of the planet painted from purely theoretical considerations, especially concerning its temperature. A non-reflecting body situated at the distance of Mars from the Sun, would have a temperature of −22° F. As the temperature of the Earth has been experimentally found to be very near its computed amount, there is little reason to doubt the value computed for Mars. If the Earth were as far from the Sun as our fellow planet it would receive only about 43 per cent of its present supply of heat and, according to Professor Poor, such a reduction would plunge the whole of our globe into perpetual winter. There is no evidence to prove that such a state of affairs exists on Mars. Assuming that the white maps at the poles are composed of snow we have the

visual evidence of their being formed in the planet's winter and of their melting in the summer season. That they melt as well as evaporate seems to be proven by the appearance of a dark blue band which is seen to border the caps as they diminish. This phenomenon is best accounted for by supposing it to be water which is formed in pools by the melting snow. During the summer season the polar caps of the Earth never become as small as the caps of Mars. That of the Martian southern hemisphere has been observed to entirely disappear during the planet's summer. Owing to the dearth of water-vapor in the atmosphere the deposit of snow at the poles is much lighter than that at the poles of the Earth. The absence of an ice-packed polar sea, also the greater rapidity of evaporation under the lesser force of gravity on the surface of Mars would probably assist in the more rapid and complete disappearance of its polar snows.

Water vapor, carbonic acid gas, ozone and hydro-carbons in the Earth's atmosphere prevent the rapid radiation of the Sun's heat into space. According to Dr. Arrhenius the Earth's atmosphere contains about 0.03 per cent by volume of carbonic acid gas. If deprived of this the surface temperature would fall about 21°, and this would so diminish the amount of water vapor in the atmosphere that another and almost equal fall of temperature would result. There is little doubt that the atmosphere of Mars contains a comparatively small per cent of water vapor. That the heat received during the day is lost to a great extent during the night seems to be shown by the aspect of certain parts of the planet's disc. Such regions as Hellas and Hesperia in the south temperate zone even in the summer season are usually very white as they emerge from the night, often becoming dusky as they near the noon meridian, suggesting, as also do many other parts of the planet, the appearance of heavy frosts, formed in the night and vanishing only under the noon Sun. Visual evidence thus seems to suggest that the mean temperature of Mars is much below that of the Earth, but the atmosphere of the planet surely possesses some quality which saves the possible Martians from such intense cold as one would expect to experience upon a world so situated.

A slight increase in the per cent of carbonic acid gas in the Earth's atmosphere would tend to equalize the temperature between different portions of the Earth's surface. Thus there would be less difference between the torrid, temperate and

frigid zones. Dr. Arrhenius says that the oceans of the Earth, by absorbing carbonic acid, act as regulators for the atmosphere. According to his figures the oceans take up five-sixths of the carbon dioxide artificially produced. Since Mars has no large bodies of water to act in this capacity we would expect to find a greater per cent per volume of carbonic acid gas in its atmosphere than in that of the Earth, if the sources of carbon dioxide are proportionately equal.

The Martian atmosphere exerts a pressure of less than two pounds to the square inch on the surface of the planet, and this is less dense by about one-half than the air on the tops of the Earth's highest mountains. Although so attenuated the atmosphere of Mars is perceptible almost at once to the student of the Martian markings. The dark areas emerging from it near the western limb when the planet is near opposition and therefore less gibbous, suggests the appearance of forests seen through a lifting fog. It is the atmosphere of Mars which causes the brilliant illumination all around the edge of the disc. A careful examination of the yellow regions has shown that they are usually slightly lighter near the center of the disc, gradually darkening toward the planet's limb. Thus the atmosphere seems to absorb the light reflected from the brilliantly illumined deserts, just as a vast stretch of white sand on a beach becomes dusky in the distance. A point is reached near the planet's limb where the light reflected from the atmosphere is brighter than that reflected from the surface below it, hence the brilliant illumination of the extreme edge of the disc.

Even without being able to see and to study all the complex details discovered by Schiaparelli and Lowell, the much discussed canals, Mars affords plenty of material for the earnest student of nature. We may never know whether or not the planet is inhabited, but our curiosity is aroused. The enchantment which "distance lends" and its mystery will bring many devotees to this field of research and perhaps some day, from the tangle of anomalous facts may come a solution to the problem which will be adopted by all factions of the warring astronomers.

ALPHA HYDROGEN IN THE STAR D.M. + 30° 3639.

MONROE B. SNYDER.

The splendid discovery by Dr. Walter S. Adams, of the Mount Wilson Solar Observatory, of the existence of a new line λ 4068, of unknown origin, which extends outward from the star D.M. + 30° 3639 about 4″, and thus almost equals the extent of Campbell's celebrated "hydrogen envelope" of 5″, permitted the writer* recently to point with confidence to the identity of this new radiation. The agreement in wave length between the stellar line and the proposed double line of alpha hydrogen, or of "secondary hydrogen" as it is commonly named, could then, however, be verified only, to within 1.0 A. U. The intensities of the lines were, indeed, quite favorable to the identification. But the real basis for the inference made by the writer was the fact that the supposed alpha hydrogen line obeyed the laws of radio-action, or of explosive radio-activity, which he had verified, but not published. It is now possible, without recourse to new theory, to prove the identity of the stellar line with the specified double line of alpha hydrogen with an accuracy approximating to 0.01 A. U.

Before adducing the evidence, it should be stated that it is quite significant both as to the character of the solar corona, and as to that of the "envelope" of the star D.M. + 30° 3639 that the line λ 4068 appears among the coronal lines photographed by Sir Norman Lockyer† in the total solar eclipse of January 22, 1898, and, indeed, in "Group I" which is supposed to record lines of the same type as the main coronal line, λ 5303.35, whose own identity with alpha hydrogen has already been indicated.‡

A fine series of photographic observations of the spectrum of Campbell's "hydrogen envelope" star, as reported by Mr. J. C. Duncan in Lick Observatory Bulletin No. 182, pp. 58-59, gives an accessible and, I think, decisive approval of the writer's identification of alpha hydrogen in that star. Mr. Duncan photographed the line λ 4068 in the star D. M. + 30° 3639, on three occasions at wave-lengths 4068.41, 4068.30, and 4068.58, respectively. The mean of these measurements is λ 4068.43.

* Pop. Astron. Vol. XIX, p. 170.
† Phil. Trans. Roy. Soc. Vol. 197 A p. 207.
‡ Loc. cit.

Moreover, the mean positions of the alpha hydrogen lines λ 4069.5 and λ 4067.0, each of the intensity 6, as derived from the measurements of Watson,* Hasselberg, and Ames are λ 4068.42, λ 4068.48, and λ 4068.37, respectively. The mean of these values is λ 4068.43.

The agreement between the mean position of the line in the star and the laboratory value for the line holds within 0.01 A. U. And the approach to identity, between the mean wave-length of the alpha hydrogen double line mentioned, and the wave-length of this new line in Campbell's coronal star, is seen to be proportional to the increased accuracy, available in measurements of the stellar line.

The line λ 4068 thus becomes the first to be proved as luminescent in a star, for the alpha series, or "many-lined" series, of hydrogen. It is, however, not the only important stellar line claiming kinship with alpha hydrogen.

Philadelphia Observatory, Mar. 7, 1911.

THE SUN-PATH MODEL.†

JOSEPH F. MORSE.

The Sun-Path Model is an adjustable device for demonstrating the apparent diurnal path of the Sun above and below the horizon of any latitude at the equinox and two solstice dates. It consists of a horizon disc, upon which compass directions are marked, encircled by three sun-path rings, the middle ring representing the diurnal path of the equinox Sun, and the outer rings the daily course of the Sun at solstice times—midsummer and midwinter—as viewed from center of disc, when it has been oriented with the points of compass.

The equinox ring is of the same diameter as the disc, and is pivotally secured to the east-and-west diameter of disc by pins which bisect the ring. One half of the ring is thus always above the disc, and the portion above the disc always connects with the east and west points of disc, whatever the slant of the ring to the disc. Secured to the disc in this way,

* Proc. Roy. Soc. Vol. 182 A. p. 189.

† Invented by Prof. Joseph F. Morse, Hyde Park High School, Chicago, Ill.

the equinox ring can evidently be adjusted to show the equinox path of the Sun, as viewed from any latitude. It is placed vertical to the disc for the equator, leaned southward from the

THE SUN-PATH MODEL.

vertical position for northern latitudes, and northward for southern latitudes, as many degrees in each case as the latitude considered is from the equator. The adjustment is made by a quadrant of the same radius as the ring.

The two solstice rings are of a diameter equal to the chord of disc drawn parallel to a diameter at a distance of 23½° from it. They are firmly secured to the equinox ring, parallel with it, one on either side, at a distance of 23½° from it, as measured on the disc when the three rings are vertical to disc. Thus mounted, the ring to the north will show the diurnal path of the June solstice Sun, and the southern ring the daily path of the Sun at the December solstice, for any and every latitude to which the equinox ring may be adjusted in the manner just described.

The position of the Sun's sky-path at other times of the year than equinox and solstice can be located approximately by imagining a ring placed parallel with the given rings at

the proper interval between them for the time of year in question—the given rings being three months apart. The yearly movement of the Sun can be pictured in a general way by imagining one of the solstice rings moved to the position of the other and back while kept parallel throughout with its original position. In reality, because of its combined daily and yearly movements, the Sun passes from one solstice position to the other along a fine threaded helix, or screw-thread coil, each thread of the coil representing the Sun's path for one day.

When vertical to the disc in the equator adjustment the rings show the equinox and solstice sun-paths as viewed from the equator. The equinox Sun is seen to rise due east, pass directly overhead, and set due west, while the June solstice Sun keeps 23½° to the north of the equinox Sun in its rising and setting points and noon position, and the December solstice Sun 23½° to the south. The equator is seen to have perpetual equinox, because in the vertical adjustment all the rings are bisected by the disc.

As the rings are leaned southward from the vertical position in adjusting for northern latitudes, the equinox ring continues to be bisected by the disc, and to connect with the east and west points of disc—because pivoted to its east-and-west diameter. But the June ring is thrown more and more above the disc, and the December ring to the same extent below it—showing that increase of latitude northward lengthens the days at the June solstice, and shortens them by an equal amount at the December solstice. The time of sunrise and sunset in midsummer and midwinter, giving the length of longest and shortest days at each latitude considered, is read from the intersection of time marks on the outer rings—fifteen minutes apart—with the horizon plane, indicated by a fine line bisecting the edge of the disc. The rings are fastened to their common support at the six o'clock points, morning and evening. At their noon and midnight points the rings are connected by wires representing portions of the celestial meridian, as viewed from center of disc. The southward leaning of the rings for northward adjustment also brings the day portion of the June ring nearest the zenith, and of the December ring farthest from it, and throws the rising and setting points of the midsummer and midwinter Sun, as represented by the points of intersection of outer rings with edge of disc, farther from the east and west, the June solstice Sun crossing the horizon more and more to the north, and the December Sun to the south. The

precise direction of sunrise and sunset at the solstice dates is read from the points of intersection of the outer surface of the solstice rings with degree marks on the circumference of the disc.

In the adjustment for the arctic circle the June ring just touches the north point of the disc from above, showing a twenty-four hour day, with the Sun on the northern horizon at midnight, and the December ring just grazes the south point of of the disc from below, indicating a twenty-four hour night, with the Sun just showing on the southern horizon at noon.

Beyond the arctic circle the midsummer and midwinter Sun—as represented by the outer rings—clear the horizon in their daily circuit of the sky by an increasing space—showing an increasing period of continuous sunshine in summer, and continuous night in winter.

In the adjustment for the north pole the equinox ring encircles the horizon, and the solstice rings are parallel with it, the June ring 23½° above, and the December ring 23½° below. It takes six months for the Sun to coil its way up from the horizon to the June position and back, and six months to wind its way below the horizon to the December position and return.

The adjusting of the model to southern latitudes throws the December ring increasingly above the horizon, and the June ring below it, reversing the northern seasons, except that the June Sun rises and sets more and more to the north and the December Sun to the south, as when going northward from the equator.

By showing the path of the Sun during the night, from sunset to sunrise, the model demonstrates the position of the Sun at midnight—how many degrees it is below or above the horizon and in what direction—and shows why the twilight periods lengthen with approach to the poles.

The latitude required for any given length of solstice day can be determined from the model. Suppose one wishes to know at what latitude there is a twenty-hour day at the June solstice. He adjusts the model so as to throw twenty hour-spaces of the June ring above the disc, and then reads the latitude by noting how many degrees from the zenith the noon point of the equinox ring is, using the adjusting quadrant to determine this.

In a similar way one can find the latitude at which the solstice Sun rises and sets a given number of degrees—say 60°—from the east and west.

Without further detailed description it is evident that three

rings of proper size and relation to each other, mounted to encircle a disc in the manner described, can be adjusted to demonsrate the equinox and solstice sun-paths for any latitude, and can be manipulated to answer any question that may be asked regarding the length of day and the time and place of sunrise and sunset at different latitudes at the solstice dates—problems that require the use of trigonometry for their mathematical solution. For example: How long is the midsummer day at Paris? Dawson City? Cape Town? What latitude has a thirteen-hour day at the summer solstice; a fourteen-and-a-half-hour day; a seventeen-hour day? At what latitude does the Sun rise at two o'clock in midsummer? Set at four o'clock in midwinter? Where does the June solstice Sun rise as seen from 45° north latitude? At what latitude does the midwinter Sun set 50° south of west?

The model can also be used to show the relation of the horizon of any latitude to the celestial sphere, since the equinox sun-path is identical with the celestial equator, and the solstice sunpaths are parallels of declination.

The reason for the shifting of the Sun's sky-path with the seasons and varying with latitude, in the manner shown by the Sun-path model, can be demonstrated, in a general way, by pinning a small pasteboard disc, crossed by north-to-south and east-to-west diameters, and having its circumference further subdivided into degree spaces, to a globe at different latitudes, and rotating the globe in right position to the Sun, or artificial light, for the equinox and two solstice dates. The direction of sunrise and sunset will be opposite to that taken by the pin's shadow when first and last seen on the disc, in the course of ten globe's rotation and the varying height of the noon Sun will be indicated by the varying length of the pin's shadow when disc is in the noon position.

The model is designed to supplement globes and tellurians in the study of seasons. Globe demonstration of seasons shows Earth and Sun relations as viewed from space, leaving the Sun's course as seen from different latitudes to be calculated. The Sun-path model pictures the Sun's sky-path directly as viewed from any place on the Earth at the equinox and solstice dates.

THE PROBABLE ERRORS OF ADJUSTED MEASUREMENTS.

LE ROY D. WELD.

Every person at all familiar with methods of precision in any department of science knows that no value of a quantity obtained by measurement is exactly correct, but is effected by errors of observation arising from various causes. One can easily convince himself of this by measuring a long line several times with a foot-rule, the only evidence needed being that the several results do not agree. A person measuring a quantity with all possible care may be considered as aiming at its true value in much the same way as a marksman aims at a point on a target; sometimes the shot strikes on one side, sometimes on the other, sometimes so very near to the point aimed at that, to all intents and purposes, it may be said to have struck it.

The compensating character of accidental errors of observation is so well understood that nearly any one will take the average of a set of similar measurements upon the same quantity and regard it as more reliable than any single measurement, as, in fact, the *most probable value* obtainable from the measurements. It is not difficult to show that the proceeding is justifiable in accordance with the elementary theory of errors.

Due allowance having been made for the different degrees of trustworthiness attributable to the several observations by assigning to them *weights* and repeating each result, in taking the average, a number of times equal to its weight, the question arises, how reliable is the set of observations as a whole? In our ignorance of the true value sought, the best we can do is to use this "weighted" average, and to judge the precision of the work by the amount of scattering shown in the results with respect to it. The difference between any one of a set of observed values and the most probable value obtained from the set is called the *residual* for the observation in question, and may be either positive or negative. It is from the size of the residuals that we can judge the quality of the results.

The quantity used in the theory of errors to express the precision of a set of observations is the *probable error*. Designated by ϵ, the probable error of any observation is defined as *a value such that the probability that the given observation differs from the truth by an amount numerically less than* ϵ

is equal to the probability that it differs by an amount numerically more than ϵ. The probable error of the average of several observations is similarly defined. Thus, if the mass of an object, obtained by weighing, is expressed in grams as 24.830726 ± 0.000014, we mean, not that the measured result cannot be more than fourteen millionths of a gram out of the way, but that the error is just as likely to be within this limit as beyond it, ± 0.000014 gram is then the probable error of the weighing.

If there are n observations on one quantity, with assigned weights $w_1, w_2, \cdots w_n$, the residuals being denoted by $\rho_1, \rho_2, \cdots \rho_n$, the theory of errors gives, as the probable error of an observation of unit weight, the following:

$$\epsilon = 0.6745 \sqrt{\frac{\Sigma (w \rho^2)}{n-1}}, \qquad (1)$$

where $\Sigma (w\rho^2) = w_1 \rho_1^2 + w_2 \rho_2^2 + \cdots + w_n \rho_n^2$. The same theory gives us the law, that *the probable errors of weighted observations in the same set are to each other inversely as the square roots of their weights:*

$$\frac{\epsilon_1}{\epsilon_2} = \frac{\sqrt{w_2}}{\sqrt{w_1}}. \qquad (2)$$

Then for an observation of weight w, the probable error is

$$\epsilon_w = \frac{\epsilon}{\sqrt{w}} = 0.6745 \sqrt{\frac{\Sigma(w\rho^2)}{w(n-1)}}. \qquad (3)$$

In very many cases arising in scientific work, the quantities to be determined are not measured directly nor separately, but are bound up with each other in the same measurements. In algebra instead of being told directly that $x = 3$, $y = 5$, we may be given the same information indirectly in such form as

$$\begin{aligned} 2x + 3y &= 21 \\ 9x + \ y &= 32. \end{aligned}$$

Likewise, the information regarding desired quantities may be obtained from measurements in just such forms, instead of directly. An example of this in astronomy is found in the use the zenith telescope for the determination of latitude. In this case, the quantities to be determined are the zenith distances of two stars at transit. The sum of their zenith distances is obtainable at once from their declinations; the difference is

measured by means of the micrometer on the zenith telescope. Numerous cases of the sort will occur to the astronomer and the physicist.

Though the number of equations necessary for determining l unknown quantities is l, the number of "observation equations," representing measurements which involve these unknowns, will usually be more than l. For example, the writer recently determined the *two* constants of an equal arm balance by means of ten measurements upon the sensibility at different loads, which gave rise to *ten* linear observation equations. It is one of the most important offices of the *method of least squares* to reduce such a set of n equations, involving l unknowns, to l *most probable equations* called *normal equations*, which may then be solved for the most probable values of the unknowns by algebra. The theory does not belong here, but a brief statement of the procedure, in the usual case that the observation equations are of the first degree, may be useful to those not already familiar with least squares.

Let the n observations be expressed in the form of n first degree equations involving the l unknowns q', q'', - - - $q,^{(l)}$ thus:

$$\left.\begin{array}{l} a_1 q' + b_1 q'' + \text{ - - - } + r_1 q^{(l)} = S_1 \\ a_2 q' + b_2 q'' + \text{ - - - } + r_2 q^{(l)} = S_2 \\ \text{- - -} \quad \text{- - -} \quad \text{- - -} \quad \text{- -} \\ a_n q' + b_n q'' + \text{ - - - } + r_n q^{(l)} = S_n \end{array}\right\} \qquad (4)$$

To reduce these to the l normal equations, first, let a weight w, as 2, 3, 10, etc., be assigned to each observation, and let each equation (4) be repeated a number of times equal to the weight assigned to it, so that it will count that many times in the reduction. (If the observations are regarded as equally trustworthy, the weights may all be unity). The new set will consist of Σw equations instead of n. Second, multiply each of these Σw equations through by the coefficient of q' in it (the a's in (4)). Third, add the resulting equations together, term by term. The result is the *normal equation* with respect to q'. Proceed in like manner with the coefficient of q'' (the b's), giving the normal equation with respect to q''; and so on until the last of the l normal equations has been formed.

These normal equations may be symbolized thus:

$$\left.\begin{array}{l} A_1 q' + B_1 q'' + \text{ - - - } + R_1 q^{(l)} = \Sigma\, awS \\ A_2 q' + B_2 q'' + \text{ - - - } + R_2 q^{(l)} = \Sigma\, bwS \\ \text{- -} \quad \text{- - -} \quad \text{- - -} \quad \text{- - - -} \\ A_e q' + B_e q'' + \text{ - - - } + R_e q^{(l)} = \Sigma\, rwS \end{array}\right\} \qquad (5)$$

in which $\Sigma\, awS = a_1 w_1 S_1 + a_2 w_2 S_2 + \text{ - - - } + a_n w_n S_n$, etc., and the coefficients, A_1, B_1 etc. have been determined as just explained.

The equations are now to be solved for q', q'', $\cdots q^{(l)}$ by the usual processes of algebra. The results will be the most probable values of these unknown quantities obtainable from the observations. A little consideration will show that the practice of taking the average of a set of observations on a single quantity is merely a simple special case of the procedure above described.

We now come to the question of finding the probable errors of the values thus calculated, and it was with that problem in mind that this article was especially written. Complaint has justly been made that this matter is not very clearly presented in many of the text-books on least squares. The method here suggested is one which the writer has for several years used in his classes. It is based on the same principle as other methods—that of the propagation of error.

The residuals in the observations represented by equations (4) are the discrepancies found between the first and second members of those equations when the most probable values of the unknowns q', q'', $\cdots q^{(l)}$ are substituted in the first members. These residuals having been calculated, the probable error of any one of the observation equations having a weight w is given by the formula, based on the theory of errors and commonly accepted,

$$\epsilon_w = 0.6745 \sqrt{\frac{\Sigma(w\rho^2)}{w(n-l)}}. \qquad (6)$$

(It is easily seen that (3) is a special case of (6) in which $l=1$). In some kinds of work, as in time observations with transit and clock, the probable error of an observation of unit weight may be known to the observer through long experience with his instruments, and need not be calculated. At any rate, we shall suppose the probable errors of S_1, S_2 $\cdots$ S_n in equation (4) to be known and designate them by ϵ_1, ϵ_2, $\cdots$ ϵ_n. We are now required to find the probable errors of q', q'', $\cdots q^{(l)}$, which may be designated by ϵ', ϵ'', $\cdots \epsilon^{(l)}$.

It is shown under the laws of propagation of error, that if

$$Q = k_1 x + k_2 y + k_3 z + \cdots,$$

in which the k's are constant coefficients, the probable error, E, of Q is given by the relation,

$$E^2 = k_1^2 \epsilon_x^2 + k_2^2 \epsilon_y^2 + k_3^2 \epsilon_z^2 + \cdots,$$

ϵ_x, ϵ_y, ϵ_z, - - - being the respective probable errors of x, y, z, - - - . Therefore, the probable error of the first member of the first normal equation (5) is given by

$$E_1^2 = A_1^2\,\epsilon'^2 + B_1^2\,\epsilon''^2 + \cdots + R_1^2\epsilon^{(l)2} \qquad (7)$$

while that of the second member is given by

$$\begin{aligned} E_1^2 &= a_1^2\,w_1^2\,\epsilon_1^2 + a_2^2\,w_2^2\,\epsilon_2^2 + \cdots + a_n^2\,w_n^2\,\epsilon_n^2 \\ &= \Sigma\, a^2\,w^2\,\epsilon^2. \end{aligned} \qquad (8)$$

Since these are equal, and corresponding relations exist for the remaining normal equations, we may write the following l equations:

$$\left.\begin{array}{l} A_1^2\,\epsilon'^2 + B_1^2\,\epsilon''^2 + \cdots + R_1^2\,\epsilon^{(l)2} = \Sigma\, a^2\,w^2\,\epsilon^2 \\ A_2^2\,\epsilon'^2 + B_2^2\,\epsilon''^2 + \cdots + R_2^2\,\epsilon^{(l)2} = \Sigma\, b^2\,w^2\,\epsilon^2 \\ \cdots \quad \cdots \quad \cdots \quad \cdots \\ A_e^2\,\epsilon'^2 + B_e^2\,\epsilon''^2 + \cdots + R_e^2\,\epsilon^{(l)2} = \Sigma\, t^2\,w^2\,\epsilon^2 \end{array}\right\} \qquad (9)$$

These equations are of the first degree in ϵ'^2, ϵ''^2 - - -, $\epsilon^{(l)2}$ and may be readily solved for these values, the required probable errors themselves being then obtained by extracting the square roots.

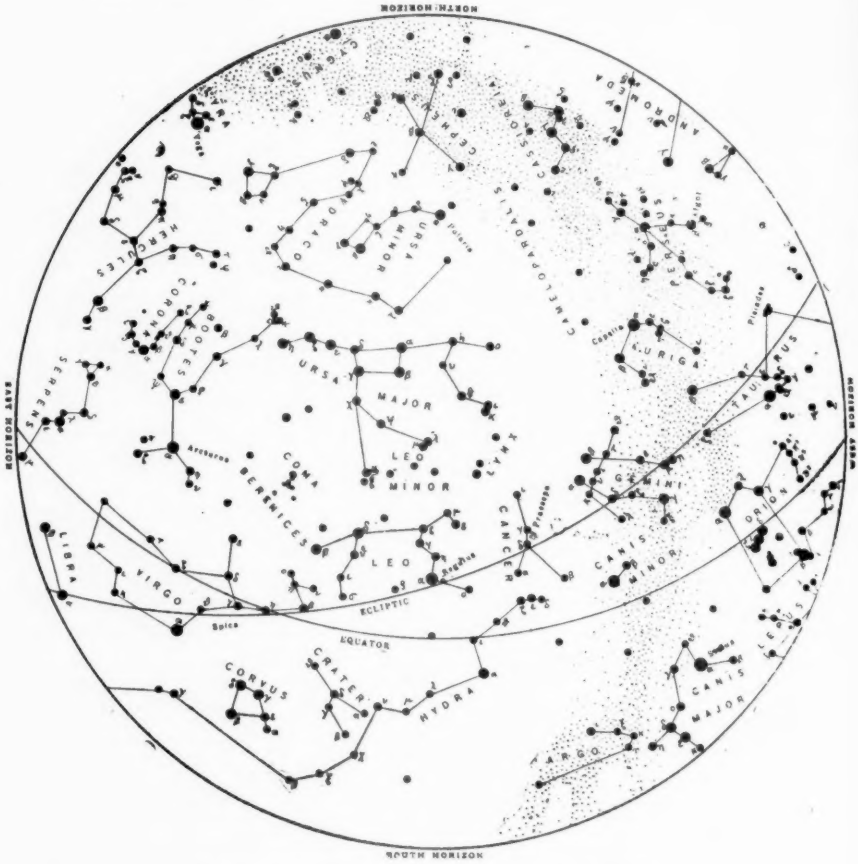
The actual process is usually not so complicated as might be supposed. The number of unknowns is seldom large, often only two or three. The quantities A, B, etc. appearing in (9) are already known from the normal equations. The weights w are simple numbers, frequently all unity. The work is greatly facilitated by the use of tables of squares and square roots. And it may finally be remarked, that since no great precision is required in determining probable errors, they being intended only to give a general idea of the reliability of the observational work, superfluous decimal places may be dispensed with in the several stages of their calculation.

Coe College, Cedar Rapids, Iowa.
March 6, 1911.

PLANET NOTES FOR MAY, 1911.

Mercury, after its greatest elongation east on April 14, will move westward rapidly, and will pass the Sun on May 5, being then at inferior conjunction. It will then continue to move westward and will reach a distance of about 23° west of the Sun by the last of the month. It will then rise about an hour and a half before the Sun, and will be a morning star. It will reach its greatest distance from the Sun in miles on May 16, being then in aphelion.

During this month Mercury will move eastward, then westward, then eastward again in Aries. It will not be very brilliant because of the phase, less than half of the illuminated surface being visible.



THECONSTELLATIONS AT 9:00 P. M., APRIL 1, 1911.

Venus will move eastward through the constellations, Taurus and Gemini, during the month of May, keeping from 40° to 45° east of the Sun. It will therefore continue to be the bright evening star and will be a little higher in the western sky each succeeding night. It will reach its greatest heliocentric latitude north on May 21. Venus may easily be recognized because of its striking brilliancy in the western sky after sunset. On May 31 it will be about 20° north of Procyon, a star of 0.5 magnitude.

Mars will move eastward in Aquarius. It will be a few degrees south of the equator during the month, and will rise about four hours before the Sun.

It will be in conjunction with the Moon on May 22. It will be distant from the Earth approximately one and one half times the distance from the Earth to the Sun, hence will not be very brilliant.

Jupiter will rise before sunset in this month and will be the brightest star in the eastern sky in the early evening. It will be moving westward very slowly in the constellation Libra. It will be about 12° south of the equator. It is approaching the Earth slowly, and will be about four and a half times as far as the mean distance of the Sun from the Earth during this month. The semi-diameter of the disk on May 17 will be 21″.15.

Saturn will move eastward slowly in the constellation, Aries. It will not be favorably situated for observations during this month because of its proximity to the Sun. During the latter part of the month it will reappear in the morning sky just before sunrise.

Uranus describes a short path in Sagittarius. It crosses the meridian about two hours before sunrise rather low in the south.

Neptune may be found in the constellation Gemini, about 15° north of Procyon. It will cross the meridian in the afternoon and will be rather low in the western sky at sunset during the latter part of the month. It cannot be seen except in a relatively large telescope.

Occultations visible at Washington.

Date 1911	Star's Name	Magnitude.	IMMERSION. Washington M.T. h m	IMMERSION. Angle f'm N. °	EMERSION. Washington M.T. h m	EMERSION. Angle f'm N °	Duration h m
May 5	57 B Leonis	6.5	8 10	66	8 51	359	0 41
6	46 Leonis	5.8	5 0	131	6 20	290	1 19
9	38 Virginis	6.1	6 20	171	7 12	262	0 52
12	25 Librae	6.0	8 50	188	9 21	239	0 31
13	57 B Scorpii	5.7	10 37	106	12 1	310	1 24
13	27 G Scorpii	5.8	13 0	31	13 13	14	0 14
13	41 G Scorpii	6.3	16 26	142	17 22	238	0 56
15	X Sagittarii	4.4	9 14	118	10 20	276	1 6
15	10 G Sagittarii	5.7	15 2	60	16 21	303	1 19
18	40 B Capricorni	6.2	12 11	64	13 22	283	1 10
19	35 Capricorni	6.0	14 34	17	15 22	305	0 48

Phenomena of Jupiter's Satellites.

Central Standard Time.

1911	h	m				1911	h	m			
May 4	13	48	II	Oc.	Dis.	7	7	24	I	Tr.	In.
5	12	58	I	Tr.	In.		7	33	I	Sh.	In.
	13	4	I	Sh.	In.		9	35	II	Tr.	Eg.
	15	9	I	Tr.	Eg.		9	45	I	Sh.	Eg.
	15	17	I	Sh.	Eg.	8	6	58	I	Ec.	Re.
6	7	46	III	Ec.	Re.	12	14	42	I	Tr.	In.
	8	28	II	Tr.	In.	13	9	17	III	Oc.	Dis.
	8	43	II	Sh.	In.		10	21	III	Oc.	Re.
	10	12	I	Oc.	Dis.		10	24	III	Ec.	Dis.
	11	0	II	Tr.	Eg.		10	41	II	Tr.	In.
	11	19	II	Sh.	Eg.		11	16	II	Sh.	In.
	12	30	I	Ec.	Re.		11	45	III	Ec.	Re.

1911	h	m			
May 13	11	56	I	Oc.	Dis.
	13	14	II	Tr.	Eg.
	13	53	II	Sh.	Eg.
	14	24	I	Ec.	Re.
14	9	8	I	Tr.	In.
	9	28	I	Sh.	In.
	11	19	I	Tr.	Eg.
	11	40	I	Sh.	Eg.
15	8	27	II	Ec.	Re.
	8	52	I	Ec.	Re.
20	12	33	III	Oc.	Dis.
	12	56	II	Tr.	In.
	13	40	I	Oc.	Dis.
	13	44	III	Oc.	Re.
	13	50	II	Sh.	In.
	14	22	III	Ec.	Dis.
21	10	53	I	Tr.	In.
	11	22	I	Sh.	In.
	13	4	I	Tr.	Eg.
	13	34	I	Sh.	Eg.
22	7	28	II	Oc.	Dis.
	8	6	I	Oc.	Dis.
	10	46	I	Ec.	Re.
	11	4	II	Ec.	Re.
23	7	31	I	Tr.	Eg.
	8	3	I	Sh.	Eg.
28	12	39	I	Tr.	In.
	13	17	I	Sh.	In.
29	9	47	II	Oc.	Dis.
	9	52	I	Oc.	Dis.
	12	40	I	Ec.	Re.
	13	41	II	Ec.	Re.
30	7	6	I	Tr.	In.
	7	46	I	Sh.	In.
	9	17	I	Tr.	Eg.
	9	58	I	Sh.	Eg.
31	6	55	II	Tr.	Eg.
	7	9	I	Ec.	Re.
	8	7	III	Sh.	In.
	8	18	II	Sh.	Eg.
	9	44	III	Sh.	Eg.

Note.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse; Oc., occultation; Tr., transit of the satellite; Sh., transit of the shadow.

COMET NOTES.

Search Ephemeris of Comet Wolf:—I send you a search ephemeris of comet Wolf, 1898-1911, for next summer. This comet was discovered by Wolf of Heidelberg, passed its perihelion July 4, 1898. It was not seen at the time it passed its perihelion in 1905 on account of its unfavorable position with regard to the Earth and Sun. The next perihelion passage will take place February 23, 1912. The comet will be in opposition the first week of July next (1911), and will be nearest to the Earth about August 1, 1911.

Greenwich Midnight 1911		α h	m	s	δ °	′	″	Log r	Log Δ
July	18	18	20	16	+15	13	45	0.41020	0.23741
"	22	18	16	49	+15	7	25	0.40604	0.23528
"	26	18	13	41	+14	56	27	0.40186	0.23386
"	30	18	10	40	+14	39	28	0.39766	0.23334
Aug.	3	18	8	0	+14	18	41	0.39340	0.23333
"	7	18	5	52	+13	53	59	0.38908	0.23392
"	11	18	4	5	+13	25	24	0.38474	0.23508
"	15	18	2	44	+12	53	33	0.38034	0.23670
"	19	18	1	48	+12	18	51	0.37594	0.23883
"	23	18	1	28	+11	41	48	0.37146	0.24130
"	27	18	1	34	+11	2	41	0.36694	0.24406
"	31	18	2	8	+10	21	25	0.36240	0.24725
Sept.	4	18	3	16	+ 9	40	6	0.35782	0.25041
"	8	18	4	52	+ 8	57	28	0.35320	0.25387
"	12	18	6	58	+ 8	14	18	0.34856	0.25746

F. E. SEAGRAVE.

Seabreeze Fla., Feb. 23, 1911.

Preliminary Search Ephemeris of Comet 1852 IV, (Westphal). The following note and ephemeris are taken from *Astronomische Nachrichten* No. 4475, contributed by Adolf Hnatek. The accompanying search ephemeris of the comet, Westphal, extends through the spring and summer of 1911,

and thus forms a continuation of the positions given for a similar purpose in *A. N.* Vol. 186, p. 107. The relative brightness of the comet will, during the coming summer, be very favorable for finding the comet, as may be seen from the following table which is prepared on three different assumptions as to the length of the period.

Period	60 years	61 years	62 years
	m	m	m
1911 April 10	11.4	12.6	13.4
June 19	10.7	12.2	13.1
Aug. 28	10.3	12.1	13.1

In the mean time the comet has not only moved considerably nearer the Sun, but it will besides this, in all probability, be in opposition, which last fact reduces its distance from the Earth by about one and one half times the radius of the Earth's orbit. If one adds to the magnitudes given in the table about two, it follows that, in the most unfavorable case of a period of sixty two years, it would even then have a magnitude of about fifteen, which might lead to its discovery during the summer of this year, because of the favorable posi- of the comet on the one hand, and on the other because of the long nights in the southern hemisphere favorable for making longer exposures.

PRELIMINARY EPHEMERIS OF COMET WESTPHAL.

Period 60 years

1911		α		δ		log *r*	log Δ
		h	m	°	′		
April	10.5	20	41.2	−58	6.3	0.7346	0.7352
	20.5		47.7	58	51.0	0.7297	
	30.5		52.5	59	44.1	0.7247	0.7026
May	10.5		55.2	60	44.9	0.7196	
	20.5		55.6	61	52.1	0.7144	0.6715
	30.5		53.1	63	3.6	0.7090	
June	9.5		47.3	64	15.1	0.7035	0.6421
	19.5		38.0	65	22.9	0.6980	
	29.5		24.7	66	20.5	0.6922	0.6200
July	9.5	20	8.3	67	1.4	0.6864	
	19.5	19	49.7	67	19.4	0.6803	0.6055
	29.5		30.7	67	11.9	0.6742	
Aug.	8.5	19	13.0	66	38.6	0.6678	0.6004
	18.5	18	58.2	65	42.6	0.6613	
	28.5		47.3	64	29.2	0.6546	0.6037
Sept.	7.5	18	40.7	−63	3.8	0.6478	

Period 61 years.

1911		α		δ		log *r*	log Δ
		h	m	°	′		
April	10.5	19	12.5	−64	17.8	0.8647	0.8531
	20.5		12.9	65	9.8	0.8621	
	30.5		11.1	66	3.3	0.8594	0.8331
May	10.5	19	6.7	66	55.9	0.8566	
	20.5	18	59.6	67	45.2	0.8539	0.8152
	30.5		49.8	68	27.6	0.8511	
June	9.5		37.5	68	59.7	0 8483	0.8015
	19.5		23.4	69	18.7	0.8454	
	29.5	18	8.4	69	22.0	0.8425	0.7935
July	9.5	17	53.6	69	9.1	0.8395	
	19.5		40.1	68	41.2	0.8365	0.7916
	29.5		29.7	68	1.0	0.8334	
Aug.	8.5		21.0	67	10.8	0.8303	0.7953
	18.5		14.2	66	13.1	0.8272	
	28.5		10.8	65	19.0	0.8240	0.8028
Sept.	7.5	17	10.7	−64	12.9	0.8207	

Period 62 years

		α h	α m	δ °	δ ′	log r	log Δ
April	10.5	18	12.1	−65	52.6	0.9438	0.9294
	20.5		9.2	66	36.3	0.9421	
	30.5	18	4.2	67	16.0	0.9403	0.9146
May	10.5	17	57.1	67	53.0	0.9385	
	20.5		47.7	68	21.9	0.9370	0.9025
	30.5		37.1	68	41.4	0.9349	
June	9.5		25.3	68	49.6	0.9331	0.8940
	19.5		13.0	68	45.8	0.9312	
	29.5	17	1.1	68	29.7	0.9293	0.8907
July	9.5	16	50.5	68	2.3	0.9274	
	19.5		41.5	67	26.2	0.9255	0.8922
	29.5		34.5	66	43.4	0.9236	
Aug.	8.5		29.7	65	56.4	0.9216	0.8978
	18.5		27.1	65	7.6	0.9196	
	28.5		26.4	64	19.0	0.9176	0.9960
Sept.	7.5	16	27.7	−63	32.0	0.9156	

Wien, February 1911.

VARIABLE STARS.

Three New Variable Stars:—In *A. N.* 4474 Professor W. Ceraski announces the discovery by Mme L. Ceraski of three new variable stars, 6.1911 Trianguli, 7.1911 Piscium and 8.1911 Piscium.

6.1911 Trianguli.

	α	δ
1855.0	$1^h\ 47^m\ 7^s$	33° 4′
1900.0	1 49 43	33 17

Mr. S. Blazko concludes from a study of twenty five photographs taken between 1906 and 1911 that the star varies between magnitudes 11 and 12 and that its period is short.

7.1911 Piscium.

	α	δ
1855.0	$0^h\ 51^m\ 53^s$	+ 27° 9′
1900.0	0 54 18	27 24

Seventeen photographs taken between 1907 and 1910 indicate a variation from magnitude 10½ to less than magnitude 12. The period is probably long.

8.1911 Piscium.

	α	δ
1855.0	$1^h\ 4^m\ 24^s$	21° 27′
1900.0	1 6 49	21 41

From a study of fourteen photographs taken between 1908 and 1911 Mr. Blazko finds that the variation of this star is between magnitudes 9½ and 12. The period is long and the last maximum occurred during November 1910.

New Variable Star 9.1911 Lacertae:—In *A. N.* 4476 Mr. M. Luizet announces the variability of BD + 51°3414 (1855.0). In the Bonn catalogue, the recorded magnitude is 9.5. On February 15, 16, 20 and 21 the light was 1.5 magnitudes less than that of BD + 51°3416 ($9^m.4$); hence the star must have been about magnitude 11. On photographs of the region surrounding Nova Lacertae, taken by Wolf on January 4 and published in *A. N.* 4466, the two stars BD +51°3414 and BD + 51°3416 are practically the same magnitude. The star BD + 51°3414 must therefore have diminished by about 1.5 magnitudes between January 4 and February 21. The star is probably a long period variable.

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68 U Herculis:—In the February number of *L'Astronomie*, Professor H. E. Lau has published an interesting note on the variable star, 68 U Herculis. The matter which follows is substantially a translation of Professor Lau's note. This star is a binary system composed of a star of magnitude 5 and a companion of magnitude 10 at a distance of 4″. The principal star varies between magnitudes 4.6 and 5.4 in an irregular period. According to the observations of Schmidt at Athens, this star seems to represent no known type of variable star. Near minima the star passes through a phase marked by very rapid eruptions; near maxima, on the contrary, the light varies in a regular way. Schmidt's light curve is composed of a sine curve of 40 days and a second wave of 12 hours, the amplitude of which depends upon the phase of the main curve.

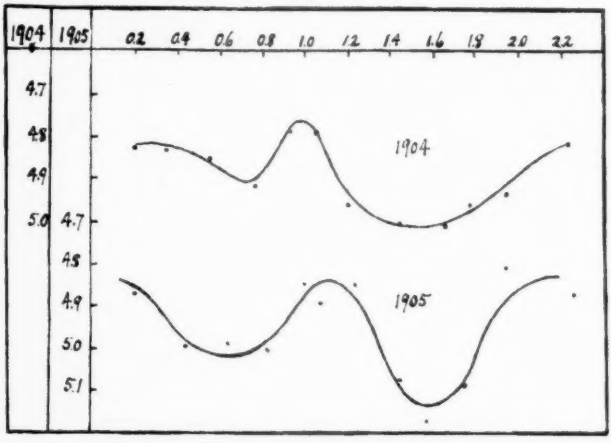
The spectrum of 68 U Herculis being of the Orion type, Professor Lau regarded the star as a photometric double. The secondary wave shown by Schmidt might be attributed to the influence of the parallactic angle, but this supposition would not explain the relation between the amplitude of this wave and the phase of the variable. Professor Lau observed the star very carefully from June 1904 to January 1906, but his observations did not confirm his previous ideas as to the nature of the system. The period came out either 36 or 73 days, but very often he found the star faint at epochs of maxima computed from Schmidt's period. The star seemed to belong to the irregular variables. But by grouping the observations Professor Lau was led to recognize a law governing the changes of the star; every five or six months, the irregularities repeat themselves in the same order.

The discussion of measures of the radial velocity of 68 U Herculis made by Dr. R. Baker (Publ. of Allegheny Observatory, No. 11) furnished a clue to the mystery. The spectrograms of the Allegheny observatory show that the period is 2.0510 days only. The phases of the light curve reproduce themselves at the same hour every 41 days. If we observe the brightness of the variable at the same hour we get an apparent period of 41 days, the value derived by Schmidt, Schwab, Yendell, and Luizet. In the winter months, we are forced to observe the star either in the morning or evening so that the winter observations do not reveal any trace of maxima at the calculated times. When we recommence to observe the star near midnight in the spring, the maxima reappears at the epochs calculated. So the irregularities in 68 U Herculis are only apparent. At Athens, Schmidt was able to follow the star for six hours, thanks to the short duration of twilight in Greece, so that he was able to recognize the existence of a very short period without being able to postulate the true law of the star's variation.

Using the period of Dr. Baker, Professor Lau traced the new light curve of 68 U Herculis. He finds the following mean for 1904 and 1905, assuming as an original epoch January 1.0, 1905 Paris Mean Time.

Observations of 1904			Observations of 1905		
Phase d	Magnitude	Nights	Phase d	Magnitude	Nights
0.20	4.84	5	0.21	4.87	5
0.35	4.83	5	0.44	4.99	5
0.55	4.85	5	0.64	4.98	5
0.78	4.92	5	0.82	5.00	5
0.93	4.78	5	1.01	4.84	5
1.06	4.79	5	1.08	4.89	5
1.21	4.97	5	1.24	4.84	5
1.46	5.00	5	1.46	5.08	5
1.67	5.01	5	1.58	5.17	5
1.78	4.96	5	1.74	5.09	5
1.96	4.93	5	1.93	4.80	11

The two light curves are represented in the figure, that of 1905 being displaced 0.3 magnitude in order to separate the series of points. We see that 68 U Herculis belongs to the β Lyrae type and varies between magnitudes 4.80 and 5.10 with a secondary minimum of 4.95.



LIGHT CURVE OF 68 U HERCULIS.

According to the spectroscopic observations this star is composed of two stars with identical spectra, the masses of which are seven and three times that of the Sun. From the nature of their spectra, the stars seem to be in the same stage of development; we can assume then that their densities are very nearly the same. Under this supposition, we find for the diameters of the two components, 6,000,000 and 4,000,000 kilometers, so that the mean density of the system is only 0.1, the density of the Sun being taken as unity.

The spectrum of 68 U Herculis merits a minute study. The absence of brilliant rays shows that the companion is denser than that of β Lyrae. The

study of the spectrum of this system will enable us to decide whether the famous triplets in the spectrum of β Lyrae result from absorption phenomena or are actually of electro-magnetic origin.

Variable Stars of the δ Cephei Type:—In the *Comptes Rendus* for January 9, 1911, Mr. M. Luizet gives a note on the variability in brightness of certain stars of the type of δ Cephei, and states that he has devised a method of determining the orbits of some of these stars without the aid of their radial velocities as determined by the spectroscope. He says that the Cepheids may be separated into two groups; in the one there is in the light-curve of the variable either an arrest of the diminution of light or a secondary maximum; in the other there is no indication of the secondary maximum. The second group he again divides into five classes: (1) The minimum is near the middle of the period and the variation of brightness is of about the same rapidity at maximum and minimum; (2) the minimum is near the middle of the period but the variation is more rapid near the minimum than near the maximum; (3) the minimum is still near the middle of the period but the variation is more rapid at maximum than at minimum; (4) the diminution of brightness is slower than the increase; (5) the increase of brightness is very rapid, while the diminution quite rapid just after maximum, slows up in such a way that during a considerable fraction of the period the brightness is almost constant.

The principal spectroscopic and photometric data which we have, upon which to base any explanation of these variables, are four: (1) The spectra of the cepheids are always single; (2) the period of the binary system deduced from measures of radial velocity is the same as that derived from the change in brightness; (3) according to Dr. Sebastian Albrecht the maximum and minimum of brightness coincide practically with the greatest velocity of approach and recession respectively; (4) the variation of light is continuous and in a given period there is but one maximum and one minimum.

Luizet adopts the following hypotheses as the basis of his solution of the problem: (1) The maximum and minimum of brightness are produced as the luminous body passes the ascending and descending nodes the average brightness occuring at the extremities of the focal chord which is perpendicular to the line of nodes; (2) the luminous body is sensibly spherical and by some external cause, for example a resisting medium, the portion of the body which is in advance in its orbital motion is rendered more luminous than that which is behind; (3) the two points of the light-curve, a half period apart, having brightness $M-e$ and $m+e$ respectively, correspond to periastron and apastron.

The following elements calculated by Luizet show that his method compares well with that which makes use of the radial velocities.

	Period	Eccentricity		Periastron	
		Luizet	Spectroscopic	Luizet	Spectroscopic
	d			d	d
ζ Geminorum	10.15	0.14	0.22	9.8	6.4
SU Cygni	3.85	0.39	0.21	3.7	2.5
T Vulpeculae	4.44	0.47	0.43	4.2	3.8
δ Cephei	5.37	0.30	0.46	5.1	4.8

The time of periastron is reckoned from the epoch of maximum. When the elements have been computed for any of the five classes of cepheids the brightness at any moment may be calculated by the formula,

$$E = S + S' + (S - S') \cos (\lambda - V),$$

in which E is the brightness at the given moment, 2S and 2S′ are the maximum and minimum brightnesses, λ the longitude of periastron reckoned from the ascending node and V the true anomaly.

The Astronomische Gesellschaft Catalogue of Variable Stars.— In *Knowledge* for February 1911, Mr. Felix deRoy, honorary secretary of the "Societé d'Astronomie d'Anvers" gives the following account of the catalogue of variable stars which is being prepared in Germany.

"Some years ago the 'Astronomische Gesellschaft', represented by its committee on variables, decided to compile and publish a great catalogue of these objects. As will be seen afterwards, works of this kind have been planned and partly printed elsewhere, but it may be confidently assumed that the bibliographical task now undertaken in Germany will go far beyond all that has been done till to-day, and will form a standard work, whose value, as it approximates to completeness, can hardly be overrated.

"The catalogue will be divided in four great sections:— (a) Variable stars; (b) suspected variables; (c) Variable stars in clusters; (d) New or temporary stars; dealing with one thousand objects. Other somewhat abridged monographs will deal with newly discovered variables, for which the available material of observation is less abundant. The work will give for each variable the 'complete' bibliographical sources and references, and a thorough discussion of all published (and, in many cases, unpublished) data. It will not include, however, some well-known variables on which complete monographs have already been published, such as Mira, *o* Ceti, (Guthwick), χ Cygni (Rosenberg), δ Libræ (Kron), R Coronæ (Ludendorf) and U Geminorum (van der Bilt).

"The discussion of this formidable material, always kept up to date, has been divided among a number of fellow-workers. At the time of writing, the manuscript for seven hundred stars is ready, two hundred are under discussion, and one hundred are still in abeyance. In the course of its twenty-third meeting, recently held in Breslau (see Knowledge 1910, November and December), the Society voted a sum of 1,000 marks (£50) towards the continuation of the work, and 10,000 marks (£500) towards the cost of printing the first part which may be expected to appear in the course of the present year."

Approximate Magnitudes of Variable Stars on Mar. 1, 1911.

[Communicated by the Director of Harvard College Observatory, Cambridge, Mass.]

Name.	R.A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R.A. 1900. h	m	Decl. 1900. °	′	Magn
χ Androm.	0	10.8	+46	27	<14	Y Cephei	0	31.3	+79	48	<13
T Androm.		17.2	+26	26	9.2 *i*	U Cassiop.		40.8	+47	43	11.3 *i*
T Cassiop.		17.8	+55	14	9.0	RW Androm.		41.9	+32	8	12.8*d*
R Androm.		18.8	+38	1	9.8*d*	RV Cassiop.		47.1	+46	53	10.1

Approximate Magnitudes of Variable Stars on Mar. 1, 1911—Con.

Name.	R. A. 1900. h	m	Decl. 1900. °	′	Magn.	Name.	R. A 1900 h	m	Decl. 1900 °	′	Magn.
W Cassiop.	0	49.0	+58	1	9.5	U Gemin.	7	49.2	+22	16	13.8
S Piscium	1	12.4	+ 8	24	<13	V Cancri	8	16.0	+17	36	10.5*d*
S Cassiop.		12.3	+72	5	<10.4	RT Hydrae		24.7	— 5	59	8.8*d*
R Piscium		25.5	+ 2	22	8.0 *i*	U Cancri		30.0	+19	14	<14
RU Androm.		32.8	+38	10	10.5 *i*	S Hydrae		48.4	+ 3	27	10.0*d*
Y Androm.		33.7	+38	50	13.0*d*	T Hydrae		50.8	− 8	46	8.5 *i*
R Arietis	2	10.4	+24	35	8.0	T Cancri		51.0	+20	14	10.0*d*
Z Cephei		12.8	+81	13	<13.5	W Cancri	9	4.0	+25	39	8.0
o Ceti		14.3	− 3	26	9.2*d*	R Leo. Min.		39.6	+34	58	8.5*d*
S Persei		15.7	+58	8	8.4	R Leonis		42.2	+11	54	10.4
RR Persei		21.7	+50	49	13.0	R Urs. Maj.	10	37.6	+69	18	7.9
RR Cephei		30.4	+80	42	13.0*d*	W Leonis		48.4	+14	15	<13.5
R Trianguli		31.0	+33	50	6.0	S Leonis	11	5.7	+ 6	0	12.0
W Persei		43.2	+56	34	9.6	R Comae Ber.		59.1	+19	20	13.4
U Arietis	3	5.5	+14	25	8.0	T Can. Ven.	12	25.2	+32	3	11.0*d*
X Ceti		14.3	− 1	26	9.5*d*	T Urs. Maj.		31.8	+60	2	12.8
Y Persei		20.9	+43	50	10.5*d*	RS Urs. Maj.		34.4	+59	2	13.0*d*
R Persei		23.7	+35	20	8.0 *i*	S Urs. Maj.		39.6	+61	38	8.2
R Tauri	4	22.8	+ 9	56	9.0	T Urs.Min.	13	32.6	+73	56	13.5
W Tauri		22.2	+15	49	8.8	S Bootis	14	19.5	+54	16	9.8
T Camelop.		30.4	+65	57	8.8	R Camelop.		25.1	+84	17	8.8
RX Tauri		32.8	+ 8	9	11.5 *i*	R Coronae	15	44.4	+28	28	7.1 *i*
X Camelop.		32.6	+74	56	8.4	R Draconis	16	32.4	+66	58	11.3*d*
R Orionis		53.6	+ 7	59	10.4	S Herculis		47.4	+15	7	9.0
V Orionis	5	0.8	+ 3	58	13.6*d*	R Ophiuchi	17	2.0	−15	58	10.5
R Aurigae		9.2	+53	28	10.0 *i*	RY Lyrae	18	41.2	+34	34	14.2
S Aurigae		20.5	+34	4	9.8	V Lyrae	19	5.2	+29	30	10.2
W Aurigae		20.1	+36	49	10.6	S Lyrae		9.1	+25	50	<14
S Camelop.		30.2	+68	45	10.8*d*	U Draconis		9.9	+67	7	11.7*d*
U Aurigae		35.6	+31	59	10.0 *i*	χ Cygni		46.7	+32	40	<11
SU Tauri		43.2	+19	1	10.0 *i*	RS Cygni	21	9.8	+38	28	9.0
Z Tauri		46.7	+15	46	13.0 *i*	R Delphini		10.1	+ 8	47	<10
U Orionis		49.9	+20	10	11.0	U Cygni		16.5	+47	35	9.0
V Camelop.		49.4	+74	30	<14	RZ Cygni		48.5	+46	59	<11
Z Aurigae		53.6	+53	18	10.5	R Vulpeculae		59.9	+23	26	10.0*d*
X Aurigae	6	4.4	+50	15	10.6*d*	X Cephei	21	3.6	+82	40	<12
SS Aurigae		5.8	+47	46	<13:5	T Cephei		8.2	+68	5	10.2
V Monoc.		17.2	− 2	9	12.4 *i*	S Cephei		36.5	+78	10	9.3*d*
U Lyncis		31.8	+59	57	14.4*d*	SS Cygni		38.8	+43	8	11.8
S Lyncis		35.9	+58	0	11.0*d*	S Lacertae	22	24.6	+39	48	9.0
X Gemin.		40.7	+30	23	13.0*d*	Nova Lacertæ		31.8	+52	12	9.5*d*
Y Monoc.		51.3	+11	22	13.7*d*	R Lacertae		38.8	+41	51	11.0*d*
R Lyncis		53.0	+55	28	9.2*d*	R Pegasi	23	1.6	+10	0	11.5*d*
V Can. Min.	7	1.5	+ 9	2	14.2*d*	V Cassiop.		7.4	+59	8	13.0*d*
R Gemin.		1.3	+22	52	11.6*d*	ST Androm.		33.8	+35	13	9.3
R Can. Min.		3.2	+10	11	8.8	Z Cassiop.		39.7	+56	2	10.2 *i*
RR Monoc.		12.4	+ 1	17	13.2*d*	RR Cassiop.		50.7	+53	8	13.0
V Gemin.		17.6	+13	17	8.0 *i*	R Cassiop.		53.3	+50	50	8.0 *i*
S Can. Min.		27.3	+ 8	32	9.3						

The letter *i* denotes that the light is increasing; the letter *d*, that the light is decreasing; the sign <, that the variable is fainter than the appended magnitude. The above magnitudes have been compiled by Mr. Leon Campbell of the Harvard College Observatory from observations made at the Amherst, Vassar, Mt. Holyoke, Olcott, Jacobs, Swartz Hunter, and Harvard, Observatories.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Greenwich Mean Time beginning with noon. To reduce to Central Standard time subtract 6 hours, or for Eastern time subtract 5 hours, etc. For stars marked with an * alternate minima are given; ** every third minimum; † every tenth minimum.]

*U Cephei

	d	h
May	1	3
	6	3
	11	2
	16	2
	21	1
	26	1
	31	1

*Z Persei

	d	h
May	3	22
	10	0
	16	3
	22	6
	28	9

RY Persei

	d	h
May	6	21
	13	18
	20	15
	27	11

**RZ Cassiop.

	d	h
May	3	6
	6	20
	10	10
	14	1
	17	15
	21	5
	24	19
	28	9
	31	23

*ST Persei

	d	h
May	3	6
	8	14
	13	21
	19	4
	24	11
	29	18

RX Cephei

	d	h
May	2	6

*Algol

	d	h
May	2	1
	7	19
	13	12
	19	6
	25	0
	30	17

**RT Persei

	d	h
May	3	10
	5	23
	8	12
	11	2
	13	15
	16	4
	18	17
	21	6
	23	19
	26	9
	28	22
	31	11

λ Tauri

	d	h
May	1	17
	5	16
	9	15
	13	14
	17	12
	21	11
	25	10
	29	9

*RW Tauri

	d	h
May	4	13
	10	2
	15	15
	21	4
	26	17

*RV Persei

	d	h
May	3	9
	7	8
	11	6
	15	5
	19	4
	23	3
	27	1
	31	0

RW Persei

	d	h
May	3	4
	16	9
	29	14

RS Cephei

	d	h
May	7	23
	20	9

*RY Aurigae

	d	h
May	1	12
	6	23
	12	10
	17	21
	23	7
	28	18

*RZ Aurigæ

	d	h
May	2	9
	8	9
	14	10
	20	11
	26	12

51.1908 Gemin.

	d	h
May	2	8
	6	8
	10	8
	14	8
	18	9
	22	9
	26	9
	30	9

*RW Gemin.

	d	h
May	2	11
	8	4
	13	22
	19	16
	25	9
	31	3

*U Columbæ

	d	h
May	5	5
	10	19
	16	10
	22	0
	27	15

*RW Monoc.

	d	h
May	4	16
	8	11
	12	7
	16	2
	19	22
	23	17
	27	13
	31	8

RX Gemin.

	d	h
May	9	13
	21	18

**RU Monoc.

	d	h
May	2	12
	5	5
	7	21
	10	14
	13	6
	15	23
	18	15
	21	8
	24	0
	26	17
	29	9

**R Canis Maj.

	d	h
May	4	6
	7	15
	11	1
	14	11
	17	21
	21	7
	24	16
	28	2
	31	12

RY Gemin.

	d	h
May	2	7
	11	14
	20	21
	30	5

*Y Camelop.

	d	h
May	2	2
	8	16
	15	7
	21	22
	28	12

RR Puppis

	d	h
May	5	21
	12	7
	18	17
	25	4
	31	14

**V Puppis

	d	h
May	3	16
	8	0
	12	9
	16	18
	21	2
	25	11
	29	20

†X Carinæ

	d	h
May	6	7
	11	17
	17	3
	22	12
	27	22

S Cancri

	d	h
May	6	14
	16	2
	25	14

S Velorum

	d	h
May	5	19
	11	17
	17	16
	23	14
	29	13

**Y Leonis

	d	h
May	3	14
	8	16
	13	17
	18	19
	23	20
	28	21

**RR Velorum

	d	h
May	3	11
	9	0
	14	14
	20	3
	25	17
	31	6

*SS Carinae

	d	h
May	7	7
	13	21
	20	11
	27	1

RW Urs. Maj.

	d	h
May	6	9
	13	17
	21	2
	28	9

**Z Draconis

	d	h
May	4	0
	8	2
	12	3
	16	5
	20	7
	24	9
	28	10

*SS Centauri

	d	h
May	2	4
	7	3
	12	2
	17	1
	22	0
	26	23
	31	22

*δ Libræ

	d	h
May	4	4
	8	20
	13	11
	18	3
	22	19
	27	11

*U Coronæ

	d	h
May	7	13
	14	11
	21	9
	28	6

*SW Ophiuchi

	d	h
May	4	18
	9	16
	14	13
	19	11
	24	8
	29	5

*SX Ophiuchi

	d	h
May	1	15
	5	18
	9	21
	14	0
	18	3
	22	6
	26	9
	30	12

R Aræ

	d	h
May	1	8
	5	19
	10	5
	14	15
	19	1
	23	11
	27	22

Minima of Variable Stars of the Algol Type.—Continued.

****U Ophiuchi**

	d	h
May	1	22
	4	10
	6	23
	9	11
	12	0
	14	12
	17	0
	19	13
	22	1
	24	14
	27	2
	29	14

****SZ Herculis**

May	1	17
	4	4
	6	14
	9	1
	11	12
	13	23
	16	10
	18	21
	21	8
	23	19
	26	6
	28	17
	31	4

Z Herculis

May	2	8
	6	8
	10	8
	14	7
	18	7
	22	7
	26	7
	30	7

SX Draconis

May	2	20
	8	1
	13	5
	18	9
	23	14
	28	18

***RS Sagittarii**

May	4	4
	9	0
	13	20
	18	16
	23	12
	28	7

***V Serpentis**

	d	h
May	7	18
	14	16
	21	14
	28	12

†RZ Draconis

May	2	13
	8	1
	13	13
	19	1
	24	13
	30	2

**** RX Herculis**

May	1	20
	4	12
	7	4
	9	20
	12	12
	15	4
	17	20
	20	12
	23	4
	25	20
	28	12
	31	4

***SX Sagittarii**

May	2	5
	6	9
	10	12
	14	16
	18	20
	22	23
	27	3
	31	7

***RR Draconis**

May	1	12
	7	4
	12	19
	18	11
	24	3
	29	19

****U Scuti**

May	2	6
	5	3
	8	0
	10	20

****U Scuti**

	d	h
May	13	17
	16	14
	19	11
	22	7
	25	4
	28	1
	30	22

***RX Draconis**

May	2	16
	6	11
	10	5
	14	0
	17	19
	21	14
	25	9
	29	4

***RV Lyræ**

May	6	15
	13	20
	21	1
	28	5

16.1908 Vulpec.

May	3	6
	7	17
	12	5
	16	16
	21	4
	25	15
	30	3

***U Sagittae**

May	5	4
	11	22
	18	16
	25	11

***Z Vulpec.**

May	4	16
	9	14
	14	12
	19	10
	24	8
	29	6

SY Cygni

	d	h
May	4	12
	10	12
	16	12
	22	12
	28	12

***WW Cygni**

May	7	9
	14	0
	20	15
	27	6

SW Cygni

May	1	18
	6	8
	10	22
	15	11
	20	1
	24	15
	29	5

VW Cygni

May	6	10
	14	20
	23	7
	31	17

***UW Cygni**

May	1	17
	8	15
	15	12
	22	10
	29	7

W Delphini

May	2	5
	7	0
	11	20
	16	15
	21	10
	26	6
	31	1

RR Delphini

May	1	11
	6	1
	10	16
	15	6
	19	20
	24	11
	29	1

RR Vulpec.

	d	h
May	4	1
	9	2
	14	3
	19	4
	24	6
	29	7

****VV Cygni**

May	1	6
	5	16
	10	3
	14	13
	18	23
	23	10
	27	20

****28.1910Cygni**

May	2	14
	5	12
	8	10
	11	8
	14	6
	17	3
	20	1
	22	23
	25	21
	28	18
	31	16

WZ Cygni

May	30	3

***RT Lacertae**

May	3	18
	8	20
	13	22
	18	23
	24	1
	29	3

***TT Androm.**

May	4	18
	10	7
	15	19
	21	8
	26	21

21.1909 Andr.

May	3	0
	7	3
	11	6
	15	9
	19	11
	23	14
	27	17
	31	20

Maxima of Variable Stars of Short Period not of the Algol Type.

Unless otherwise indicated the times of maxima only are given; and the times of minima may be found by subtracting the interval printed in parentheses under the names of the stars.

SY Cassiop.

	d	h
May	4	7
	8	9
	12	11
	16	13
	20	14
	24	16
	28	18

RW Cassiop.

(−5 19)

May	7	2
	22	2

SU Cassiop.

(−0 22)

May	3	7
	5	6
	7	4
	9	3
	11	2
	13	1
	15	0
	16	22
	18	21
	20	20
	22	19
	24	17
	26	16
	28	15
	30	14

SV Persei

May	9	0
	20	3
	31	6

RX Aurigæ

(−4 0)

May	7	6
	18	21
	30	12

SX Aurigae

May	1	7
	2	20
	4	9
	5	22
	7	10
	8	23
	10	12
	12	1
	13	13
	15	2
	16	15
	18	4
	19	16
	21	5
	22	18
	24	7

SX Aurigae

	d	h
May	25	20
	27	8
	28	21
	30	10
	31	23

SY Aurigae

May	4	8
	14	12
	24	15

Y Aurigae

(−0 18)

May	3	0
	6	21
	10	18
	14	14
	18	11
	22	7
	26	4
	30	2

RZ Gemin.

(−1 7)

May	5	7
	10	19
	16	7
	21	20
	27	8

RS Orionis

(−2 0)

May	8	10
	15	23
	23	12
	31	2

T Monoc.

(−9 23)

May	11	22

W Gemin.

(−2 2)

May	6	19
	14	17
	22	15
	30	13

ζ Gemin.

(−5 0)

May	2	16
	12	19
	22	23

RU Camelop.

(−9 12)

May	21	22

V Carinæ

	d	h
	(−2	4)
May	7	8
	14	1
	20	18
	27	10

T Velorum

(−1 10)

May	3	23
	8	14
	13	5
	17	21
	22	12
	27	3
	31	19

W Carinæ

(−1 0)

May	3	21
	8	6
	12	15
	17	0
	21	9
	25	18
	30	3

S Muscæ

(−3 11)

May	5	9
	15	1
	24	17

T Crucis

(−2 2)

May	4	21
	11	15
	18	8
	25	2
	31	19

R Crucis

(−1 10)

May	6	1
	11	21
	17	16
	23	12
	29	8

S Crucis

(−1 12)

May	1	17
	6	10
	11	2
	15	19
	20	11
	25	4
	29	20

RZ Centauri

	d	h
May	1	9
	2	8
	3	6
	4	5
	5	3
	6	2
	7	0
	7	23
	8	22
	9	20
	10	19
	11	17
	12	15
	13	14
	14	12
	15	11
	16	10
	17	8
	18	7
	19	5
	20	4
	21	2
	22	1
	22	23
	23	22
	24	20
	25	19
	26	17
	27	16
	28	14
	29	13
	30	11
	31	10

W Virginis

(−8 5)

May	10	15
	27	22

V Centauri

(−1 11)

May	5	11
	10	23
	16	11
	21	23
	27	10

R Triang.Austr.

(−1 0)

May	3	2
	9	20
	13	6
	16	15
	20	1
	23	10
	26	19
	30	5

S Triang.Austr.

	d	h
May	1	17
	8	1
	14	9
	20	16
	27	0

S Normæ

(−4 10)

May	7	12
	17	6
	27	0

RV Scorpii

(−1 10)

May	3	11
	9	13
	15	14
	21	16
	27	17

RV Ophiuchi Minimum

May	2	23
	6	16
	10	8
	14	1
	17	17
	21	10
	25	2
	29	19

X Sagittarii

(−2 22)

May	6	22
	13	22
	20	23
	27	23
	28	19

Y Ophiuchi

(−6 5)

May	15	3

W Sagittarii

(−3 0)

May	5	16
	13	6
	20	20
	28	10

Y Sagittarii

(−2 2)

May	1	12
	7	7
	13	1
	18	20
	24	15
	30	9

Maxima of Variable Stars of Short Period not of the Algol Type.-Con

U Sagittarii		SU Cygni		X Cygni		Y Lacertae		V Lacertae	
d	h	d	h	d	h	d	h	d	h
(− 2	23)	(− 1	7)	(−6	19)	(−1	10)	May 16	1
May 7	9	May 2	20	May 13	4	May 2	22	21	1
14	2	6	16	29	13	7	6	26	0
20	20	10	12	T Vulpec.		11	14	31	0
27	14	14	9	(− 1	10)	15	22	X Lacertae	
β Lyræ		18	5	May 2	17	20	6	May 2	18
(− 3	7)	22	0	7	4	24	13	8	5
(− 3	2)	25	21	11	13	28	21	13	15
May 1	12	29	18	15	23	δ Cephei		19	2
8	17	η Aquilae		20	11	May 4	3	24	13
15	9	(− 2	6)	24	21	9	12	29	23
21	16	May 4	18	29	8	14	21	SW Cassiop.	
28	8	11	22	TX Cygni		20	6	May 5	1
κ Pavonis		19	2	May 6	11	25	14	10	5
(−4	7)	26	7	21	5	30	23	15	22
May 9	2	S Sagittae		VY Cygni		Z Lacertae		21	8
18	4	(−3	10)	(−2	14)	May 3	21	26	19
27	6	May 3	15	May 3	24	14	18	RS Cassiop.	
U Aquilæ		12	0	11	20	25	15	(−1	19)
(− 2	4)	20	10	19	17	RR Lacertae		May 1	14
May 3	21	28	19	27	13	May 1	22	7	21
10	21	X Vulpec.		VZ Cygni		8	8	14	5
17	22	(−2	1)	(−1	1)	14	18	20	12
24	23	May 6	22	May 1	3	21	4	26	19
31	23	13	6	5	23	27	14	RY Cassiop.	
U Vulpec.		19	13	10	20	V Lacertae		(−7	10)
(− 2	3)	25	21	15	17	(−1	10)	May 6	19
May 6	21	V Vulpec.		20	14	May 1	3	18	22
14	20	Minimum		25	10	6	2	31	2
22	20	May 13	2	30	7	11	2		
30	19								

GENERAL NOTES.

Owing to an extraordinary sale, No's 156, June-July 1908; 161, January 1909; 169, November 1909; 170, December 1909, are out of our files. Requests for these numbers have not been numerous enough to warrant the reprinting of any of the issues, but still a number of our subscribers need them to complete their files of POPULAR ASTRONOMY. If any of our readers, therefore, have made all the use they care to of these numbers, and care to dispose of them, we should be very glad to buy them back at the rate of 30 cents a copy.

Dr Andrew W. Phillips, since 1891 professor of mathematics in Yale College and since 1895 dean of the graduate school, will retire from active service at the end of the present academic year. *Science*, Mar. 3.

Charles Joyce White, professor of mathematics at Harvard University from 1885-1894, has been appointed emeritus professor. *Science* Mar. 3.

The Astronomical Society of Pittsburg:—An astronomical society has been formed in Pittsburg with the object of bringing together the many persons in that vicinity who are interested in astronomy. The name of the society is the Astronomical Society of Pittsburg, and its head quarters are at the Allegheny Observatory. The only officers of the society are an executive committee, which is at present composed of Dr. Frank Schlesinger, Dr. Walther Riddle, and Mr. W. R. Ludewig.

A Large Meteor:—Word has come to us from Cedar Rapids, Iowa, that a very large and brilliant meteor was seen by some workmen about 2:15 A. M., Friday, March 10. According to the report the path of the meteor was toward the east a short distance south of the Moon. It left a trail of light that continued visible for some time, and appeared to be as wide as the diameter of the Sun. From the description of it, this meteor must have been unusually striking in appearance and would have attracted the attention of any one who happened to be out of doors at the time. We have not heard of it from any other source.

The Vatican Observatory:—*Knowledge* for February 1911 contains interesting account of "The Vatican Observatory of to-day" by W. Alfred Parr, illustrated by five excellent cuts. Four modern observatory domes now surmount in a line the ramparts of the ancient fortifications erected in the ninth century by Leo IV. The distance between the extreme domes is something like like a half kilometer or between a quarter and a third of a mile. Where a part of the great wall has yielded to the ravages of ten centuries, for a space of about 85 metres, a slender steel bridge, the gift of a wealthy American, replaces the masonry, thus enabling the astronomers to pass from dome to dome without "coming down to Earth." Father Hagan, formerly director of the Georgetown College Observatory, Georgetown D. C., has been director of the Vatican Observatory since 1906.

Co-operative Astronomy:—The University of La Plata and the University of Michigan have arranged for coöperation in the work of their astronomical observatories. Professor W. J. Hussey has been appointed director of the La Plata Observatory, but is still to remain director of the observatory of the University of Michigan. He will divide his time equally between the two in. stitutions, spending the second semester of each year at Ann Arbor. Mr. R. P. Lamont of Chicago is furnishing a 24-inch refracting telescope for the University of Michigan. It is the intention to take this instrument, when completed, to a favorable site in Argentina, and while it is there to have it used under the joint auspices of the two Universities. *Science*, Mar. 3.

Effective Diameters of the Stars:—In the *Comptes Rendus* for January 9, 1911 is a note by Mr. Charles Nordmann on the effective diameters of the stars. All stars are so far away that their apparent disks are too small to be perceptible with the aid of the most powerful telescope If all were of equal intrinsic brightness with the Sun it would be possible by measuring their relative brightness to calculate the real diameters of those whose distance is known. But it is known that stars differ very widely in intrinsic brightness. For example Mr. Nordmann by means of a special photometer, which determines the effective temperature of a star, has shown that the intrinsic brightness of Vega is at least 43 times as great as that of Aldebaran.

Letting r and R represent the radii of the star and Sun respectively, d and D their distances, g and G their photometric magnitudes and e and E their effective intrinsic brightness, he gives the following formula by means of which the radius of the star may be computed.

$$\log \frac{r}{R} = \log \frac{d}{D} - \left[\frac{1}{5}(g-G) + \frac{1}{2}\log \frac{e}{E}\right]$$

In the following table are given the results obtained for ten of the brighter stars:

Star	Magitude g	Parallax $''$	Effective Temperature δ	$\log \frac{e}{E}$	Effective Radii $\frac{r}{R}$
Sirius	− 1.58	0.37	12200	1.291	1.13
Procyon	+ 0.48	0 30	6810	0.494	1.35
Aldebaran	+ 1.06	0.15	3500	8.853—10	13.50
Capella	+ 0.21	0.12	4720	9.819—10	8.26
Vega	+ 0.14	0.12	12200	1.291	1.57
γ Cygni	+ 2.32	0.10	5620	0.119	0.92
α Persei	+ 1.90	0.08	8300	0.805	1.83
Polaris	+ 2.12	0.07	8200	0.786	1.93
β Andromedæ	+ 2.37	0.07	3700	9.031—10	13.0
β Persei	+ 2.10	0.05	13800	1.418	1.29

From this it would appear that Sirius, although much hotter than the Sun and intrinsically nearly twenty times as bright, is yet only a little larger in actual size, while Aldebaran, at a much lower temperature and with one-fourteenth the intrinsic brightness, has thirteen times the diameter of the Sun. Aldebaran and β Andromedæ are as many times larger than the Sun, as the Sun is larger than Jupiter or Saturn. Even this tremendous diameter at the distance of Aldebaran subtends an angle of only 0″.018 so that it would take a telescope of about 20 feet (6^m) diameter to make it directly visible.

We do not know what dependence is to be placed upon these results but they are certainly very interesting.

Report of the Committee on Fundamental Stars.—There has just reached us the latest report of the fundamental star committee in connection with the work upon the photographic chart of the sky. The stars considered in the work of this committee are divided into five classes, designated by the committee as follows:

A, fundamental stars; the 343 stars marked F in Boss' list of 1059 standard stars.

A′, fundamental stars; the other stars in Boss' list.

B, standard stars; stars in the Backlund and Hough lists.

C, intermediary stars; stars chosen, in the northern hemisphere of the sky, by Küstner and, in the southern, by Hough, in accordance with the resolution of the committee in 1909.

D, stars of reference.

The committee presents the following resolutions:

I. All observatories coöperating in the fundamental work should observe all the stars in classes A and B which cross the meridian at zenith distances less than 70°. It is desired that those who are able to do so observe also the stars in list A′. The coöperation of observatories which, for special reasons, observe only stars of the list A and A′ will be gladly accepted.

II. All coöperating observatories which undertake to observe the class C stars should observe the class B stars in the zone of which they have charge, and 2° on each side of this zone.

III. In all meridian observations of class D stars, the class C stars, in the zones and 2° on each side of them should be observed.

The committee recommends that the director of each coöperating observatory shall employ, for the solution of all questions which present themselves, the process which he thinks best under his local conditions. The fundamental observations necessarily include observations of the Sun, in order to deduce the position of the equinox, etc. All these observations should be published with the details of the calculation of results from them. It should be stated whether or not the observations of the Sun have been utilized in making up the catalogue and, if they have been so used, the method of calculation should be indicated It is important to search for a more satisfactory method of observing the Sun. To attain a little progress in the direction the committee

considers essential to the elimination of both periodic and systematic errors of the fundamental system, the addition of a constant α to the positions of all stars observed at a single observatory. This constant will be finally determined from observations of the Sun made at all the observatories.

The rest of the report is taken up with a discussion of the various errors to which the observations may be subject and with recommendations for their elimination or correction. The following is a summary of the errors considered.

IN RIGHT ASCENSION.

1. Errors due to the displacement of the optical axis by lateral flexure, or to defects in the pivots.
2. Defects in the illumination of the field.
3. Personal equation in magnitude.
4. Personal equation resulting (*a*) from the direction of apparent motion. in the field, for stars transiting north or south of the zenith and above or below the pole; (*b*) from a change in the velocity of motion in the field with the declination.
5. Diurnal variation of azimuth and level.
6. Diurnal variation of the clock.
7. Periodic errors in right ascension of the adopted fundamental hour stars.

IN DECLINATION.

1. Imperfect knowledge of the errors of graduation.
2. Errors of the micrometer screw.
3. Imperfect knowledge of flexure.
4. Personal equation of position depending upon magnitude of a star.
5. Personal equation applicable to all zenith distances.
6. Variation in latitude.
7. Imperfect knowledge of refraction; (*a*) error of constant of refraction; (*b*) errors due to the difference between exterior and interior temperature; (*c*) the variation of refraction with the season, with the hour angle and zenith distance of the Sun.

A New Easter Formula:—In *A. N.* 4473 Dr. J. Hartmann gives a new formula for computing the date of Easter which appears to be somewhat simpler than that proposed by Gauss in 1800. The method is as follows:

Letting Y represent the number of the year concerned,

in the division	$Y : 19$	let the remainder be a
" " "	$Y : 4$	" " quotient " q
" " "	$(M - 11\,a) : 30$	" " remainder " c
" " "	$(Y + q + c - D) : 7$	" " remainder " d

then easter will be on

$$\text{March } [28 + c - d].$$

The numbers M and D are constant for the Julian calendar and constant for a century at a time for the Gregorian calendar, and may be taken from the following table:

		M	D
Julian Calendar	Constant	225	0
Gregorian Calendar	1582—1699	202*	10
	1700—1799	203	11
	1800—1899	203	12
	1900—2099	204**	13
	2100—2199	204**	14
	2200—2299	205*	15
	2300—2399	206	16

An asterisk * in the column for M means that if c comes out 29 it is to be written 28 A double asterisk ** signifies that if c comes out either 29 or 28 it is to be diminished by 1.

As an illustration we compute the date of easter for 1911 as follows:

$1911 : 19 = 100 +$ $\qquad a = 11$
$1911 : 4 = 477 +$ $\qquad q = 477$
$(M - 11a) : 30 = (204 - 121) : 30 = 83 : 30 = 2 +$ $\qquad c = 23$
$(Y + q + c - D) : 7 = (1911 + 477 + 23 - 13) : 7 = 342 +$ $\qquad d = 4$

$\therefore$ Easter = March $(28 + 23 - 4)$ = March 47 = April 16.